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EXPERIMENTAL PSYCHOLOGY: A RETROSPECT[1]

By E. B. TITCHENER

Experimental psychology, as we all know, took shape against a background of physics and of a physiology informed by physics. Fechner was a physicist; Helmholtz was physicist and physiologist; Weber and Hering and Wundt were physiologists. The new science, then, was to be a science of the type of experimental physiology; Wundt called his great compendium of 1874 Principles of Physiological Psychology; and in all sorts of ways—in choice of subjects for investigation, in the manner of presentation of published results, in the psychologist's general mode of approach to experimental problems—the influence of physiology was manifest. Think of the early problems! — sensory quality and sensory intensity, the reaction experiment in its various complications, the time sense, the range and fluctuations of attention, the perception of tactual and visual space; they were one and all of them problems that had a physiological aspect and that suggested an experimental control by instruments of physiological type. Wundt's Physiological Psychology is a tremendous advance upon the Lectures of eleven years before; yet a very great deal of it is anticipated in the third edition of Wundt's own Physiology.

These things, in the large, are known to all of us, and I do not wish to spend time on the familiar. We cannot, however, if we are taking an historical view, omit facts simply because they are obvious; and besides, the facts of which I am reminding you are, in my judgment, of quite first-rate importance. I believe that experimental psychology had an extraordinarily fortunate birth. No doubt, the early investigators set to work too hastily and too confidently. No doubt, we have wasted an immense

[1]This paper contains the substance of an address delivered before the twenty-second annual [meeting of Experimental Psychologists, April 9, 1925, at the dedication of Eno Hall, Princeton University.

amount of energy on Weber's Law and Helmholtz' theory of visual sensations and the windinesses of nativism and empiricism. No doubt. But when all is said it remains true that the initial impulse was sound and the initial perspective correct. If physics and physiology and psychology—let us rather use the more general word, and say: if physics and biology and psychology cannot be housed under one roof, as members of a single family, then the term science can have no stable meaning. The accomplishment seems more difficult to us than it did to the mid nineteenth century; our store of facts is more bewildering, and we put less trust in our coordinating concepts. The ideal, nevertheless, of a biology modelled upon physics, the mother of the sciences, and of a psychology modelled upon that physically informed biology holds for us no less than it held for our fathers.

So experimental psychology, as it started on its course, was impelled by the inner urge to be a science in the manner of experimental physiology. What, now, were the external influences that bore upon it, and under what environmental conditions did it grow up? The environment, I need not say, was hostile; and if we are to 'know ourselves' as we are today,[2] we must take some account of the kind and degree of that hostility.

The first of the three major influences that wrought against the establishment of experimental psychology—for as I look back on our history, I distinguish three such influences—was local and temporal; it was the influence of Herbart and the Herbartian psychology. To most of us, I suppose, Herbart is little more than an historical name; to Wundt in the seventies Herbart was an all-pervading institutional opponent.[3] Leipzig, in particular, the university to which Wundt was called in 1875, had long been the head and centre of the Herbartian movement. Hartenstein, Herbart's editor, had it is true given up his chair some fifteen years earlier. But Ziller was still there,—Ziller, who was one of the original editors of the *Zeitschrift für exacte Philosophie*, the technical organ of the Herbartian school, and who in 1868 had founded the hardly less important *Verein für wissenschaftliche Pädagogik*. Drobisch and Strümpell, two of the greatest names among the Herbartians, still held their Leipzig chairs,—and were to hold them for some time to come, since Drobisch lived to 1896 and Strümpell to 1899. In 1875 Lazarus and Steinthal, the founders of the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* (Wundt had already had a tilt with them in 1863) were professors in Berlin; Volkmann and Lindner were at Prag,—the whole of Austria, indeed,

[2] The Delphic maxim γνῶθι σαυτόν is carved above the door of Eno Hall.

[3] It is worth remembering that the lives of the two men overlapped; Wundt was nine years old when Herbart died.

had been Herbartianised by Bonitz and Exner; Waitz was at Marburg; Stoy was at Jena. To us, truly, these men are just a list of names and dates; to a nascent experimental psychology they represented a highly formidable opposition.

It would, of course, be a very foolish critic who should say that Wundt became a voluntarist because Herbart had been an intellectualist, or that Wundt made psychology the basis of the *Geisteswissenschaften* because Herbart had severed theoretical from practical philosophy and had derived his psychology in part from metaphysics. Yes, but it would be a foolish reader, too, who should forget that all through the second part of the Physiological Psychology Wundt has Herbart steadily in mind; that the improvised doctrine of apperception is meant as a counterblast to Herbart; that the whole of Wundt's psychology beyond the chapter on perception is shaped with polemical reference to Herbart. That, you see, is the sort of trick that history plays upon us; a good many of the later Wundtians have been anti-Herbartians without realising it. So that the effect of Herbartianism upon experimental psychology was in reality twofold, internal and external. The Herbartians were in possession; they must be dislodged, superseded, discredited, if experimental psychology was to grow to power; that was the external side of things. Internally, meantime, the very fact of having to combat a well-rounded and critically tested system tinged and moulded the doctrines of experimental psychology itself. I shall not try to give you further details; let me only remind you, before we leave Herbart, that the struggle lasted over into the present century; Wundt's rejoinder to Delbrück appeared in 1901.[4]

The second unfavorable influence which I have to note is of far greater importance,—partly because of its antiquity and perennial vitality, partly because of its seeming innocence and the guise of kinship with experimental psychology which it often assumes, so that it works insidiously from within rather than openly from without. It is the influence of empirical psychology, a mode of psychology that goes back to Aristotle and Thomas Aquinas, and that forms the staple contents of most psychologies, down to and including our twentieth-century textbooks. The empirical psychologist professes to take mind as he finds it: I use the term 'mind' without prejudice, for the subject-matter of empirical psychology; in point of fact, the definition of that subject-matter has varied very greatly: always, however, the empirical psychologist professes to take mind as he finds it. And, like everybody else, he finds mind in use, whether in a man's converse with himself or in his inter-

[4] *Sprachgeschichte und Sprachpsychologie, mit Rücksicht auf B. Delbrücks "Grundfragen der Sprachforschung,"* esp. I. and II.

course with his fellow-men. This subject-matter, then, mind in use, the empirical psychologist seeks to reduce to order and arrangement; and empirical psychology thus becomes a rationalisation of practice, a 'description and explanation' of mental uses. Our sister-science of biology suffers from the same sort of contamination: I have read textbooks of physiology that were in essence nothing else than rationalisations of the art of medicine, and I have read textbooks of biology that contained little more than a 'description and explanation' of the practice of adaptation. Biology, fortunately for her, stands nearer than we to physics, and has that great example more closely before her eyes; none the less, biology has suffered. Experimental psychology has suffered immensely more.

It happened that the year 1874, which saw the publication of the Physiological Psychology, was also the year of the appearance of Brentano's Psychology from the Empirical Standpoint, —a masterly and masterful work which shows the temper of the empirical psychologist in exemplary form. Brentano's book made from the first a deep impression upon psychology, and this impression has but deepened and widened as the years have passed. It did not, however, come on the scene as a third combatant, opposed equally to the Herbartians and to experimental psychology; things were not as simple as that. On the one hand, Brentano did not think it worth while to challenge Herbart on even terms. His teacher Trendelenburg had already disposed polemically of the Herbartian metaphysics; and Brentano, when he has to speak of the Herbartian psychology, dismisses it in a few polite but very decided sentences as an obvious failure. Though, then, the Empirical Psychology gives a round dozen references to Herbart and the leading Herbartians, it deals with them only in matters of detail; it looks on the dominance of the Herbartian school as an historical accident, of no intrinsic importance; it shows nothing of the obsession that we have noted in the Physiological Psychology. Wundt, in this regard, had found an ally; but the ally showed a certain aristocratic indifference toward the common enemy. On the other hand,—and this was the real danger for experimental psychology,—neither Brentano nor Wundt saw that his own conception of psychology refused and denied the conception of his rival: that an empirical psychology, a rationalisation of mental practice, was utterly at variance with an experimental psychology that should be a science of fact and law coordinate with experimental physiology and experimental physics. Wundt still had one foot, so to say, in the empirical camp; and Brentano was ready, subject to his own view of psychological method, to adapt the procedures of experiment to his empirical purposes.

You will expect me, now, to compare and contrast the two books, the Physiological and the Empirical Psychologies. You will realise, from what I have just said, that the contrast will not be clean and sharp; there is in Wundt a great deal of empirical psychology. That apart, however, there is still a preliminary point to be made, before we can draw our parallels. Empirical psychology is not to be defined by the contents of any single book. I have called Brentano's work exemplary; but that does not mean that his doctrines would find universal acceptance, even among his fellow empiricists. For it is of the very essence of empiricism, you remember, to classify and explain the uses of mind; and that is a task which calls less for facts than for ingenuity; and where ingenuities are in play, systems will differ individually. Let me take an illustration; and I choose one in which both the men of whom I am speaking are involved. We find that the Wundt of 1874 tends to delimit psychology, as the later Wundt overtly delimits it, by point of view and not by subject-matter; there are for Wundt no psychical, there are only psychological phenomena. We find that Brentano, on the contrary, recognises psychical phenomena as a special and distinct field of subject-matter. But on this point Brentano is at one with Herbart, the metaphysical psychologist, while Wundt represents a tendency that first appears among the empirical psychologists of purest blood. The definition by point of view has its source in the empirical psychology of Locke; and it has been adopted, in our own day, by Ward and (in his later years) by James,—by the two most distinguished and most representative empirical psychologists of the English-speaking world. Should we not have expected that Brentano, steeped as he was in the traditional British psychology, would work out to the position of Ward and James rather than range himself with Herbart? The question plainly asks too much; we must not forget Brentano's training; but at any rate the illustration shows that Brentano is not 'typical of his kind.' Or let me take another instance: we tend to think of the doctrine of association of ideas as 'typically' empirical. Here again, however, Brentano would have gone his own way. He would, we know, have discussed association when he came to deal with the laws and properties of ideas,—and what he would have made of it we may, perhaps, guess in part from the treatment accorded it by Stumpf; but he would have left it behind when he was dealing with judgments and with feeling and will. Wundt, too, revolts from associationism; but the revolt is not experimentally motived. On the contrary, the doctrine of apperception is no less empirical than that of association; it bases on the 'feeling of activity'; and since activity is not a psychological quality, the 'feeling of activity' already im-

plies a rationalisation of psychological experience, quite in the empirical manner. In this instance, therefore, both Brentano and Wundt diverge, within empiricism itself, from what we are tempted to call the empirical 'type'. It is clear that not everything that Brentano says is characteristically empirical, as I have also shown you that not every difference between Brentano and Wundt is a difference between empirical and experimental psychology.

When we put the two books side by side, we find, indeed, far too much agreement. Wundt, like Brentano, accepts from the older empiricists a whole array of classificatory and explanatory terms: consciousness, attention, association,—perception, emotion, memory, imagination. If only he had had the insight to throw them all away! Wundt possessed, in fact, just one clear concept, the concept of sensation as composed of intensity and quality; and that concept was neither happily chosen nor, after its adoption, adequate to support a whole experimental psychology. All the rest were foggy from much argument; and though Wundt made heroic efforts to clarify some of them—notably in the instance of consciousness, which he sought to redefine in a strictly experimental atmosphere—they were in the end too much for him; empirical they had been, and empirical they remained. From our point of later vantage we can see that a tremendous opportunity was lost; but we have no right to think either that Wundt should have seen that opportunity or, if he had, should have thought himself strong enough or privileged enough to seize it. At any rate, nobody has been strong enough since; we ourselves are getting rid of the equivocal and theory-ridden terms piecemeal, a little at a time; and it will be long before experimental psychology is finally free of them.

The acceptance of these concepts meant, naturally, the acceptance of certain positions with which they were historically connected; and so, for example, we find Wundt as ready as Brentano to make psychology the basis of the mental sciences. It is true that Brentano is looking particularly toward logic, while Wundt has rather in mind disciplines like history, ethics, jurisprudence. It is true also that Wundt puts experimental psychology at the lower end of the psychological scale, nearest to natural science and furthest from the mental sciences. All the same, he makes psychology continuous throughout, and does not see that he changes his attitude—from experimental to empirical—as he travels upward; that is one of the reasons why the later *Völkerpsychologie* is psychologically unsatisfactory. We can only regret, once more, that his vision was not clearer, and try to take care ourselves, now that our eyes are opened,

to avoid the sort of confusion of thought that this example betrays. The task for us is very far from easy.

What, then, of the differences between the books? The differences are unmistakable. Let me remind you, to begin with, of the difference in treatment of psychological method. Three of Brentano's thirteen chapters are given over to the discussion of method. Wundt does not discuss method at all; he names it in passing, and gets on at once to the account of experimental procedures, the description of available apparatus.[5] Surely, that single difference would be enough to prove that Wundt, however imperfectly he realises what he is about, has in fact attained to a new standpoint in psychology. A science is concerned to set forth its facts and uniformities; it has no call and, as science, it has no competence to treat of method. The textbooks of the pattern sciences, experimental physics and experimental physiology, do not dream of introducing chapters on method; they leave all such preliminaries to logic, which has a place reserved for methodology; and Wundt's own *Methodenlehre* appears, accordingly, not in the Psychology of 1874, but in the Logic of 1883. Brentano, on his side, was entirely consistent. Since empirical psychology is, as I have pointed out, a work of ingenuity, since logical argumentation is of its very texture and substance, it involves a special methodology as an integral part of its construction; science is transformed into an applied logic. There is the difference; and in the effecting of the difference experimental psychology had chosen the right scientific road. Would that we might have been allowed to walk in it! But controversies began; Wundt was presently beguiled into discussing method within his psychological system; and our experimental textbooks, ever since, have done homage to logic. Remember, nevertheless, that we were started aright, and that if we ever succeed in ousting empiricism and in banishing method to its proper limbo we shall only be recovering the position that Wundt once occupied for us.

This difference, the stressing of logical method and the stressing of experimental procedure, is perhaps the most salient single difference between the two books. There are, however, plenty of other differences. Brentano, for instance, gives a long programme of psychological applications, a programme that has an extraordinarily modern tone; Wundt says not a word about applications. This difference, again, is characteristic. Brentano starts out from mind in use, and his general psychology

[5]Cf. the references to "innere Beobachtung," "reine Selbstbeobachtung," *Phys. Psych.*, 1874, 5, 8. The discussion of Kant's second objection is, evidently, methodological; but even here the argument turns on the possibility of recourse to an experimental procedure.

is, one might almost say, a theoretical applied psychology; it is, at any rate, a rationalising of general mental application; it was but natural, then, that he should look forward to special fields of application. Moreover, Brentano's general psychology is, for him, a completed psychology; however much remains to be done in detail, the principles are established,—established internationally, for psychology as a whole, now and for ever; and such a psychology is fit and ready for application. Wundt, however, does not share the confidence that is natural to the empirical psychologist; for him, psychology is just beginning; he foresees long years of patient experimentation to be spent on the establishment of principle; and application simply does not occur to him, does not show above his horizon. Or consider, again, the use that the two men make of physiology. Brentano appeals to physiology for purposes of explanation, under the principle of sufficient reason; Wundt is, in 1874, a thoroughgoing parallelist. And so the list might be lengthened. In sum, however, all differences may be traced back to the fundamental ground of divergence: Brentano is an empirical psychologist, Wundt is trying, however blindly, to escape from empiricism, and to inaugurate an experimental psychology. Brentano offers us a logical construction, built of argument and counter-argument; his bricks are the opinions of authorities, which he accepts, rejects, reshapes, in accordance with his design. Wundt seeks to put together a psychology of experimentally determined fact, and is ready to give as much space to the description of an experimental set-up as to the view of some influential empiricist. The resulting structure is ricketty enough, and is patched out all to freely with borrowed or improvised empirical materials; it represents, nevertheless, a new kind of psychology; the design, for all its hesitancies and concessions, is the design of experimental physics and experimental physiology.

I have spoken of this second influence upon our science, the influence of empirical psychology, as if it were unmitigatedly bad. So, abstractly regarded, I take it to have been. But that does not mean, let me add, that empirical psychology in and for itself is unmitigatedly bad. Not only has it shown itself, historically, the necessary forerunner of experimental psychology, but even in the days of experimental psychology it may do good service as an intermediary between science and special application. I am not here concerned with that question,[6] any more

[6] I do not myself believe that empirical psychology is adequate to this mediating function; I believe that we should be on firmer ground in logic and should also get better results in practice if we looked for mediation to a phenomenology of the 'psychophysical' organism, the entire man (cf. my *Thought-Processes*, 1909, 75). But the question is not in point here,—and obviously a footnote is not the place for its discussion.

than I am with the intrinsic merits of particular empiricists,—some of whom have done sheerly first-rate work. I am concerned with the effect of empiricism upon experimentalism; and in the abstract that effect has been wholly bad. Only, we live in the concrete; and in the concrete there are always mitigating circumstances. It was very much to the good, I said just now, that experimental psychology takes its descent from experimental physiology. Yes, but the 'psychology' of the physicist and the physiologist is almost universally an empirical psychology; that is why we see distinguished physicists running to spiritism; and you can hardly imagine a follower of Helmholtz, for example, approaching psychology in any other than empirical terms. It may well be, therefore, that the admixture of empirical psychology, which I have been condemning, was of practical utility—not only in holding Wundt's reports of experiments together in a quasi-systematic way, but also in securing friends for the new movement among the representatives of the other sciences.[7]

Such friendship, even if based in part upon misunderstanding, would help the new science in its struggle with the third environmental influence,—the hostility of philosophy. I have spoken of the Herbartians: but while in the early seventies the Herbartian psychology was dominant in Germany, it cannot be said that the Herbartian philosophy was similarly dominant. Think, for instance, of the more important men who derive from Hegel,—Erdmann at Halle (I am afraid that we do not nowadays read his delightful *Psychologische Briefe* as widely as we should), Kuno Fischer at Heidelberg, Michelet and Zeller at Berlin; and there were other schools and other distinguished and influential men. All these philosophers were, so to say on principle, opposed to experimental psychology, and Wundt's choice of the title *Philosophische Studien* for his laboratory studies reminds one of nothing so much as the small boy with thumb to nose. The philosophers were shocked at the mere idea of subjecting mind to the color-mixer and the chronoscope; and they were shocked the more as the sacrilege threatened one of their vested rights. So the controversies began,—and, really, there is a good deal in them of childish wrangling. Wundt in 1905 thought that the main battle was won, and changed to the title *Psychologische Studien*. Perhaps he was right; though the field of battle has not by any means been cleared up, and local conflicts are still going merrily on. Good time and valuable energy have, at all events, been wasted upon an intrinsically

[7] I need hardly remark that if I had been tracing the history of experimental psychology in its material rather than in its formal aspects I should have had a great deal to say of the positive contributions that physiologists have made to our science.

foolish quarrel; and there is no countervailing gain save a certain solidarity of front, a certain esprit de corps, that we should have attained in any case, under the other two hostile influences, without the added spur of philosophical antagonism. The struggle with Herbart was incidental; Herbartianism would have died a natural death in due course, and the coming of experimental psychology merely hastened the end. The struggle with empirical psychology was inevitable and has inevitably been long drawn out; for the experimental movement was, in fact, a revolutionary movement,—and, like other revolutionary movements, could grow to power and to clear vision only by way of actual conflict. The struggle with philosophy was on a lower plane than either of these others; it reflects on the one side the over-assurance of youth, and on the other the unwillingness of high priori prejudice to face the trend of events.

I have now set forth, in the oversimplified outline that is all that an address of this sort permits, what I take to be the most important facts regarding the nature and nurture of our young science. Experimental psychology came of excellent stock: a stock that had not only attained prestige in matters of the intellect, but that had also learned to frame ideals and to bear responsibilities; the birth of the science was auspicious. Its bringing up was variously chequered. The hostile influences of which I have spoken have, obviously, been unable to throttle it; otherwise we should not be here today. But indeed, its growth has been, on the whole, steady and assured. There were a few years, in the nineties of the last century, when it seemed to be outgrowing its strength; but that danger passed; and no one can now doubt that the infant of the sixties and seventies has proved itself viable. The question that remains is—What, in the year of grace 1925, is its mental age?

The question is one, I take it, that has attracted us all; and we have answered it differently, according as our mood happened to be depressed or cheerful. It is, of course, a question that we cannot properly answer at all, because—if I may use the figure—we stand somewhere within our own perspective, and therefore cannot judge either how far we have come or how far, to attain the status of the adult, we have yet to go. On the whole, I daresay that we tend to overrate our progress, perhaps on the principle that a space of time variously filled looks long in retrospect. Still, there are signs and symptoms; and we may as well pluck up our courage and try to read them. We have, I think without question, passed from infancy into childhood. Our independence of physiology is a guarantee of that: we no longer feel any necessity of consulting physiology when we lay out our investigations; we do not necessarily borrow physiological apparatus and procedures; it does not occur to us to imitate

physiology in the presentation of results; in a word, we are out of our physiological leading-strings. It is pleasant and reassuring, certainly, if while an enquiry is in course or after it has been brought to completion we can make a cross-correlation with physiology; but we feel ourselves, none the less, to be independent; we do not lean upon physiology. That sign, then, seems unequivocal; and there is another, which a bold spirit might interpret to the effect that we are approaching adolescence —I mean the radical change that has been wrought over the whole field of the science since it turned to phenomenology. We can trace the impulse to this change directly to Hering; and if Hering has had to wait a long time before coming to his own, that is partly because he was overshadowed, for almost the whole of his working life, by the counter-influence of Helmholtz, and partly because he was himself otherwise entangled in a rather crude form of empirical psychology. Phenomenology is not yet, is not of itself, experimental psychology; but it provides today a safe and sure mode of approach to the analysis of our psychological subject-matter; and our recourse to it, our realisation of its promise, may perhaps be taken as a sign of adolescence. If, then, Godfrey Thomson is right, and the intellect is most alert and most capable at the age of sixteen, we may congratulate ourselves that experimental psychology is nearing that critical point, and may expect far better things from it in the near future than have been accomplished in the past.[8]

[8]Several friendly critics among my hearers have told me that this concluding paragraph is too optimistic. I am afraid they are right; I can only plead in excuse that the occasion of the address—the dedication of the first independent building in the western hemisphere planned and erected exclusively for psychological teaching and psychological research—naturally disposed one toward optimism.

AN EXAMINATION OF KÜLPE'S EXPERIMENTS ON ABSTRACTION

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In 1904 Külpe published the results of a series of experiments on Abstraction in which the *O*s were given certain observational tasks beforehand with regard to characteristics of an object which was to be shown tachistoscopically, and on which they had to report after the exposition, mostly in answer to questions put them.

Groups of four nonsense syllables were projected on a screen for 1/8 sec. The syllables were in different colours and in different positions in different cases, but at a fixed distance from the fixation-point in the centre. Before each exposition the *O* was given the task of observing the object from a certain point of view, namely, either (1) that of deciding the total number of visible letters, or (2) that of determining the colours with their approximate position in the visual field, or (3) that of determining the figure which the syllables formed with each other, or (4) that of determining as many as possible of the single letters with their approximate position. In some cases, however, (5) "wurde. . .auch noch jedes Objekt 'ohne' Aufgabe einer rein passiven Erwartung dargeboten." Immediately after the exposition the *O* had "*zunächst die Bestimmung im Sinne der Aufgabe zu geben und dann auf meine Fragen nach den anderen Teilinhalten zu antworten.*[1] War keine Aufgabe gestellt, so musste mir die Versuchsperson zuerst über das Auskunft geben, was ihr zunächst besonders aufgefallen war, sie in erster Linie interessiert oder beschäftigt hatte, und dann gleichfalls zu meinen Fragen nach dem übrigen Stellung nehmen."[2]

As a general result from these experiments we have, according to Külpe, "dass die *meisten, richtigsten und bestimmtesten Aussagen da stattfinden, wo die Aussagen mit den Aufgaben zusammenfallen.* Die Abstraction im Sinne des Hervorhebens gewisser Teilinhalte, die *positive* Abstraction, gelingt also am besten, wo vorher eine Präoccupation des Bewusstseins, eine Prädisposition für diese Teilinhalte gegeben oder gesetzt war."[3]

Against Külpe's procedure with regard to the order in which the reports about the different characteristics of the objects were taken, Rubin advances the following. "Wenn Külpe herauszustellen wünscht wie es sich mit Antworten im Sinne der Aufgabe und den anderen Antworten verhält, um von dem, was im Gedächtnis behalten ist, auf das, was tachistoskopisch aufgefasst worden war, zu schliessen, dann muss man sein Versuchsverfahren als so unzweckmässig bezeichnen, dass es ohne

[1] The italics are ours.

[2] O. Külpe, Versuche über Abstraction, *Bericht über den I. Kongress für experimentelle Psychologie*, 1904, 57-58.

[3] *Op. cit.*, 61. The italics are Külpe's.

Beweiskraft ist. Es ist ja bekannt, dass das tachistoskopisch Aufgefasste sehr schnell aus dem Bewusstsein verschwindet. Und dadurch, dass die Bestimmungen, die nicht im Sinne der Aufgabe waren, später zur Protokollierung gelangen, sind sie in unberechenbarer Weise benachteiligt."[4]

Külpe himself was not, it would seem, unaware of this as a possible objection, since he himself states that in many cases "ein rasches Vergessen" may have affected "die Aussagen über die der Aufgabe nicht entsprechenden Teilinhalte" disadvantageously.[5] For the rest, however, Külpe does not take this into account in arriving at the general result already mentioned, except to state on the basis of his protocols "dass in den Abstraktionstatsachen unmittelbare Bewusstseinsphänomene vorliegen" and to suggest "dass gelegentlich Mängel in der sprachlichen Bezeichnung eine Rolle gespielt haben. Aber sie sind durchaus nicht schlechthin für unsere Ergebnisse in Anspruch zu nehmen, so wenig wie man Gedächtnisfehler dafür allein heranziehen darf. Die Versuchspersonen glaubten tatsächlich die Eindrücke in der angegebenen Unbestimmtheit zu *sehen*, bezw. tatsächlich keine Farbe, kein Objekt u.s.w. wahrgenommen zu haben."[6] Against these arguments from introspection which Külpe adduces in favour of his interpretation of his quantitative results, Rubin adds: "solange diese naheliegenden Annahmen nicht explicite widerlegt sind, genügen sie als Erklärung für alles was in diesen Selbstbeobachtungen steht."[7] Ach had, however, before this accepted Külpe's introspective results, reporting that he had obtained similar ones.[8] Mittenzwey had also accepted Külpe's view as to the meaning of his quantitative results;[9] and the same thing holds true of Grünbaum, it seems, for he states that "in der . . .Untersuchung von Külpe ist gezeigt, welche Bedeutung für die Abstraktion die Setzung bestimmter *Aufgaben* hat," although he himself states that Külpe's method was "eine unmittelbare Reproduktionsmethode," and "auf Grund der Reproduktion wird also über die positive Abstraktion (Hervorhebung des Teilinhaltes) geurteilt."[10] Titchener states shortly in a note that "the influence of the *Aufgabe* is. . .plainly apparent in O. Külpe's Versuche über Abstraktion."[11] Koch, in discussing Rubin's criticism of Külpe's work, remarks as against Rubin that "K.s Arbeit hat u.a. gezeigt, welche Bedeutung der Aufgabestellung zukommt, wie sie den Ablauf des Abstraktionsprozesses wesentlich bestimmt; sie diente K. durchaus nicht dazu, 'um von dem, was im Gedächtnis behalten ist, auf das, was tachistoskopisch aufgefasst worden war, zu schliessen,' denn auch das, was im Sinne der Aufgabe lag, wurde ja tachistoskopisch dargeboten. . .Die Frage, inwieweit ein rasches Vergessen bei den Aussagen der Vp. in Betracht kommt, ist von K. nur aufgeworfen, aber nicht untersucht worden."[12] It is clear that this answer does not invalidate Rubin's criticism; rather, that it unwittingly supports it; since, as has already been indicated, Külpe's

[4]E. Rubin, Bericht über experimentelle Untersuchungen der Abstraktion, *Z. f. Psych.*, 63, 1912, 386.

[5]*Op. cit.*, 66.

[6]*Op. cit.*, 67.

[7]*Op. cit.*, 387.

[8]N. Ach, *Über die Willenstätigkeit und das Denken*, 1905, 239-240.

[9]K. Mittenzwey, Über abstrahierende Apperzeption, *Psychol. Studien*, 2, 1907, 480.

[10]A. A. Grünbaum, Über die Abstraktion der Gleichheit, *Arch. f. d. ges. Psychol.*, 12, 1908, 346, 356.

[11]E. B. Titchener, *Lectures on the Experimental Psychology of the Thought-Processes*, 1909, 249.

[12]A. Koch, *Z. f. angew. Psychol.*, 7, 1913, 337.

tables of results, on which his conclusions as to the quantitative influence of the *Aufgabe* are based, were drawn up without taking the influence of memory, as affected by the order of the questions and reports, into account. It is also clear, as against Koch, that Külpe did base his view, that "eine Präoccupation im Sinne der Aufgabe" was favourable for the process of the abstraction of the characteristics of the object concerned, on the reports the *Os* were able to give after the moment of exposition, *i. e.*, on the basis of more or less immediate memory. Notwithstanding Rubin's criticism and Koch's obviously inadequate reply we find Fröbes adducing Külpe's general result given above without criticism;[13] whereas Lindworksy, in giving an account of Külpe's method, criticises it in other respects but does not mention this point.[14] Pauli gives the experiment in a somewhat modified form, in which, however, as with Külpe, "die besondere Aufgabe . . . dabei stets zuerst beantwortet [wird]."[15]

It is indeed clear that, where an *O* strictly carries out the instructions given him in the case where the observational task refers to a particular characteristic or particular characteristics of the object, a first process of abstraction, corresponding to the given observational task, takes place in direct connection with the tachistoscopic exposition and before the first question is put. In order, then, to answer the questions set him in succession after the exposition, no matter what the observational task, the *O* will have to carry out further acts of abstraction, corresponding to the different questions set, on the basis of the memory-effects of the impression created by the exposition and any abstractional processes which have already taken place. This means that, in answering the questions, he sets to work with particular schematic anticipations for the different characteristics to which the questions refer, with the result that the characteristic or characteristics concerned are brought to consciousness in the first place.[16] It is clear that we may then expect the memory-effects, on the basis of which these later processes of abstraction have to be carried out, to be fresher and so more favourable, other things being equal, for the characteristics corresponding to the observational task, than for those not corresponding to the observational task; since a shorter time, after the exposition, would have elapsed in the case of the former and less intervening disturbance would have taken place. Külpe, incorrectly, did not allow for these advantages. It is clear, then, that a re-examination of the influence of the observational task is neces-

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[13] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, II, 1920, 190-191.

[14] J. Lindworksy, Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, Abt. VI, *Methoden der experimentellen Psychologie*, Teil B, Heft 2, 1922, 168-169.

[15] R. Pauli, *Psychologisches Praktikum*, 3te Auflage, 1923, 180.

[16] Cp. O. Selz, *Zur Psychologie des produktiven Denkens und des Irrtums*, 1922, 109: "Der determinierte Abstraktionsprozess spricht nur diejenigen Abstraktionsgrundlagen an, und er führt nur die abstraktive Hervorhebung solcher Bestandstücke (selbständiger Teile oder unselbständiger Momente) der Abstraktionsgrundlage herbei, die der schematischen Antizipation des Abstraktionszieles entsprechen."

sary. The present paper gives an account of two series of experiments carried out with this aim. They were directed more especially towards the *quantitative* side with regard to the correctness of the reports of the *O*s. The experiments were carried out in the Psychological Laboratory, University of Stellenbosch, South Africa.

Series I.

In the main the method followed agreed closely with that of Külpe with regard to the nature of the objects and the time of exposition.

By means of a projection-apparatus and a rotary tachistoscope the time of exposition was made 1/8 sec., though of course it does not follow from this agreement with Külpe's exposition-time that the duration of the image was also the same.[17] The syllables fell on the radius of a circle of approximately 43 cm. and the *O* was seated at a distance of 4 m. from the screen. The 80 syllables of the 20 objects were all different, and the letters were all different in one and the same object. Five colours (white, red, yellow, green and blue)[18] were used in the present case, whereas Külpe used only four colours. The colours were all different in the case of one and the same object. The syllables were so arranged as to form a number of different figures. In the case of a number of blank experiments objects were used which differed with regard to number of letters and syllables from those already mentioned. Each diapositive was used four times in all with each *O*, the next succeeding exposition of the same diapositive being separated, for the same *O*, by at least 24 hours and 25 observations from the previous occasion on which it was used. In the case of each *O* the expositions were divided into 4 part-series, in each of which each of the objects was shown. In each of these part-series the 5 observational instructions of Külpe were given in an irregular but previously determined order. In the case of the fifth, however, the instruction was given in the following form: "Do not decide before the exposition what particular characteristic of the object to be exposed you will attend to, and attempt not to think beforehand of any particular characteristic." Experimental periods lasted for 3/4 hour or less. The observations were made in a well-darkened room. 320 observations, *i. e.*, 80 with each of the 4 *O*s were made, apart from a number of preliminary trials and 20 blank experiments with each *O* in the series itself.

The *O*s were Mr. H. F. Verwoerd, M. A. (V), a member of the staff of the Department of Psychology of the University of Stellenbosch; Mr. J. C. Conradie, M. A. (C), Mr. J. V. E. Potgieter, B. A. (P), and Mr. A. H. Murray (M), students of the same university. V and M had had previous experience as *O*s with tachistoscopic experiments.

Since, with a view to the aim of the experiments, the following is the essential point in which the procedure followed differed from that of Külpe, it is desirable to state it more fully. Immediately after each exposition the questions corresponding to Külpe's first four observational tasks were put in succession by the *E*, the next succeeding question being put as soon as

[17] Cp. W. Wundt, Zur Kritik tachistoskopischer Versuche, *Philos. Stud.*, 15, 1899, 295.

[18] The diapositives in our case were made on coloured celluloid sheets, as supplied by E. Zimmermann, Leipzig, by carefully covering the spaces round that intended for the letters with opaque white paint. The strips on which the letters were thus made were pasted over openings in squares of cardboard, the openings being at equal distances from the centre.

possible after the previous one had been answered. The order in which the questions were put after each exposition was determined by the following principles. (1) The order had to make the impression on the *O* of being a "chance" one. (2) After each of the 20 observations in each part-series, of which the results were to be used, the question corresponding to the particular one set of the first four observational tasks was put first, second, third and fourth an equal number of times. This also held for the case of "no special observational task," thus making our procedure here different from Külpe's in a further, special point. This was done with a view to equalising the conditions of answering in this respect. (3) In so far as the questions put did not correspond to the particular observational task for the exposition concerned, each type of question was also put first, second, third and fourth an equal number of times.[19]

In adducing the results bearing on the points at issue, the first question that may be put is whether there is evidence of a decreasing ability to report correctly with regard to a particular characteristic of the object concerned, within the time needed for answering all the questions, according to the increasing lapse of time and the number of other questions that had first to be answered after the exposition. In other words, does the memory-factor play a negligible rôle?

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[19]In the case of the first part-series the order followed thus became:

No. Obs.	Task	Ques. 1	Ques. 2	Ques. 3	Ques. 4
1	NL	CP	F	NL	LP
2	LP	F	CP	LP	NL
3	CP	CP	F	NL	LP
4	F	CP	F	NL	LP
5	NST	F	CP	LP	NL
6	CP	NL	LP	F	CP
7	F	F	CP	LP	NL
8	NL	NL	LP	F	CP
9	CP	LP	NL	CP	F
10	F	LP	NL	CP	F
11	NST	LP	NL	CP	F
12	CP	F	CP	LP	NL
13	LP	LP	NL	CP	F
14	F	NL	LP	F	CP
15	NL	LP	NL	CP	F
16	NST	NL	LP	F	CP
17	NL	F	CP	LP	NL
18	LP	NL	LP	F	CP
19	NST	CP	F	NL	LP
20	LP	CP	F	NL	LP

NL = number of letters, LP = letters with their positions, CP = colours with their positions, F = figure, and NST = no special (observational) task.

The order of questions for the second part-series was obtained by putting the questions of part-series I in the order 2-1-4-3, for part-series III in the order 3-4-1-2, and for part-series IV in the order 4-3-2-1. The order of the observational tasks and the diapositives remained the same as for part-series I.

TABLE I

Observ. Task / Question	Non-C CP 1	CP 2-4	Non-C LP 1	LP 2-4	Non-C NL 1	NL 2-4	Non-C F 1	F 2-4
Os	%	%	%	%	%	%	%	%
P	43.6	39.6	17.2	22.2	56.2	58.3	50	75
V	68.7	51	36.5	24.3	75	86.8	75	91.7
M	54.7	53.1	26.9	24.6	93.7	50	93.7	83.3
C	59.4	46.9	18.7	11.9	93.7	58.3	86.9	83.3

In Table I the percentages of correct answers are given in the columns. The percentages in the columns marked non-C are those obtained by the various *Os* where the observational tasks were the different non-corresponding ones (including NST). Here the percentages are those for the questions put first. In the other columns observational tasks and questions corresponded, and the percentages are the average for the cases where the questions were put second, third and fourth. In marking the reports one point was given for each colour correctly indicated and one further point where its position was also correctly indicated. The same thing was done in the case of letters and their positions.[20] One mark was also given if the figure was correctly drawn, and one if the number of letters was correctly indicated.

Table I shows that the influence of the memory-factor is of such importance that, if the question corresponding to the observational task is not put first, but put second, third and fourth, then the average percentage for these three places is actually lower, on the whole, than where the same question is put first but with a non-corresponding observational task. In other words, a result the opposite to that of Külpe is obtained as soon as the question corresponding to the observational task is given a similar disadvantageous position to that which he gave to the questions not corresponding to the observational task. It is clear, then, that the objection raised by Rubin against Külpe's procedure is shown by the experimental results to be fully justified as far as the order of the questions is concerned.

On the other hand, a further examination of the results obtained shows that it would be incorrect to conclude that the correspondence of observational task and report does not at all have the favourable type of influence on the ensuing process of abstraction that Külpe claimed for it. This is shown already by Table II.

TABLE II

Observ. task / Question	CP 1-4	non-C CP 1-4	LP 1-4	non-C LP 1-4	NL 1-4	non-C NL 1-4	F 1-4	non-C F 1-4
P	40.6	32.6	24.2	10	44	47	81.2	61.2
V	53.1	52.3	26.4	17.7	87.5	78.1	68.7	81.2
M	53.9	41.4	26.6	18.9	63	77	75	72
C	52.3	51.5	14.3	8.6	63	73.5	81.2	73.5

In the columns the percentages of correct answers are given. The numbers, after the designation of the questions, in the second row indicate that these percentages are the averages for the answers where the questions were put first, second, third and fourth.

[20] Cp., however, p. 334.

It is clear from the Table that in the cases of CP and LP the percentages are without exception higher where the observational tasks corresponded with the questions than where there was non-correspondence, although it is true that the differences are in some cases small. In the case of F we have such a higher score in three out of the four cases, but in the case of NL in no less than three out of the four cases a higher score was obtained where the observational task and question did not correspond. On especially this last, at first sight somewhat surprising result, a remark that Grünbaum makes in discussing Külpe's work has a bearing.

"Nun ist es nicht unwahrscheinlich, dass durch mehrfache, wenn auch zyklisch verteilte Ausfragen über die 'aufgabelosen' Inhalte eine 'Präoccupation des Bewusstseins' auch für sie geschaffen wurde, wenn auch in geringerem Masse, als für die durch die direkte Aufgabe bestimmten Inhalte. Nur bei einem einzelnen Versuch, dem keine vorausgehen und keine folgen, kann die spezielle Aufgabestellung und allgemeines Ausfragen die Prädisposition *ausschliesslich* für einen Teilinhalt bedeuten. Dieser Fall ist aber schwerlich bei einer experimentellen Häufung der Resultate durchzuführen. Die partielle Disposition für die 'aufgabelosen' Inhalte muss von Fall zu Fall sich ändern—dadurch wird ein variabler nicht berechenbarer Faktor eingeführt. . . ."[21] There are, however, other disturbing factors militating against the correspondence between observational task and question showing its influence clearly in the case of NL (and, to a lesser degree, in the case of F), and we shall have to return to this question.

In so far we have at least in part a corroboration of Külpe's main result. There is, however, a further point of difference to which we wish to draw attention immediately. Külpe states that "im allgemeinen kann man überhaupt finden, dass es für die Grösse und Güte der Leistungen nicht vorteilhaft war, ohne Aufgabe dem Objekt gegenüber zu stehen," although he points out that in some cases his *Os* got very good scores in this last case.[22] Since it appears, however, that the reports, for which

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[21] *Op. cit.*, 363-364.

[22] *Op. cit.*, 62. "Die eine von meinen Versuchspersonen hatte ein ausgesprochenes Interesse an der *Figur* und bestimmte diese bei fehlender Aufgabe fast ebensogut, wie bei entsprechender Aufgabe. Eine zweite wurde besonders durch die *Farben* gefesselt und hatte daneben eine starke Tendenz, die *Elemente* zu bestimmen. Bei fehlender Aufgabe leistete sie in diesen Richtungen fast ebensoviel wie bei entsprechender Aufgabe. Die dritte Versuchsperson wurde namentlich von der *Figur* und den *Farben* in Anspruch genommen, ohne jedoch bei fehlender Aufgabe in diesen Richtungen grössere Leistungen aufzuweisen." In view of the already quoted fact, however, that "war keine Aufgabe gestellt, so musste mir die Versuchsperson zuerst über das Auskunft geben, was ihr zunächst besonders aufgefallen war, sie in erster Linie interessiert oder beschäftigt hatte, um dann gleichfalls zu meinen Fragen nach dem übrigen Stellung zu nehmen," it may be accepted that Külpe's *Os* reported on these points for which they obtained high scores "bei fehlender Aufgabe" first. Apart from the fact that there was in their cases obviously a "Prädisposition" in the sense of an unconscious mental set, although perhaps not, literally, a "Präoccupation des

relatively good scores were obtained by Külpe's *Os*, notwithstanding the fact that the observation took place "ohne Aufgabe," were made first in time-order after the exposition, it is clear that they cannot be used—and, we take it, were not intended—as *real* exceptions to the general statement of Külpe's just quoted. Apart from this point, however, Külpe's own tables of results,[23] which we can only refer the reader to here,

TABLE III

Observ. task		NST	CP	LP	NL	F
Question 1-4	*Os*					
CP	P	34.4(3)	40.6(1)	32 (4)	36.7(2)	27.3(5)
	V	57.8(1)	53.1(3)	55.5(2)	49.2(4)	46.9(5)
	M	37.5(5)	53.9(1)	38.3(4)	40.6(3)	49.2(2)
	C	42.2(5)	52.3(2)	68 (1)	49.1(3)	46.9(4)
LP	P	14.2(2)	7.5(4)	24.2(1)	5 (5)	13.3(3)
	V	19 (2½)	17.2(4)	26.4(1)	16.3(5)	19 (2½)
	M	17 (5)	21.6(2)	26.6(1)	18.5(3½)	18.5(3½)
	C	7.2(4½)	7.2(4½)	14.3(1)	8.3(3)	11.6(2)
NL	P	44 (3½)	50 (2)	38 (4)	44 (3½)	56 (1)
	V	81.2(2½)	75 (4½)	81.2(2½)	87.8(1)	75 (4½)
	M	69 (3½)	69 (3½)	88 (1)	63 (5)	75 (2)
	C	75 (2½)	56.2(5)	88 (1)	63 (4)	75 (2½)
F	P	75 (2)	69 (3)	63 (4)	38 (5)	81.2(1)
	V	81.2(2½)	81.2(2½)	81.2(2½)	81.2(2½)	68.7(5)
	M	81.2(1)	69 (4)	63 (5)	75 (2½)	75 (2½)
	C	100 (1)	63 (4)	56.2(5)	75 (3)	81.2(2)

In this Table there are given first the percentages of correct answers obtained by the different *Os*, the percentages being the averages where the questions are set first, second, third and fourth for each particular observational task. Next to the percentages the figures in brackets indicate the order of goodness of the percentages concerned for the particular observational instructions and questions.

do not seem to us to offer clear evidence, as far as the figures actually adduced are concerned, for his statement. At the same time, however, conclusions to be drawn from these figures become uncertain for the further reason that we do not know at what point of time after the exposition Külpe's *Os* gave their reports on characteristics of the object, other than in the case of those reports the scores of which Külpe explicitly singles out as being relatively good notwithstanding absence of the "Auf-

Bewusstseins" for the characteristics favoured by them, it is clear that their answers to these points were favoured by the time at which they were answered. It is thus, again, the matter of the time-order of the reports which prevents us from using these results of Külpe to come to a comparison as to the relative influence of a special observational task and no special observational task.

[23] *Op. cit.*, 59, 63.

gabe." In addition to this, we are in similar ignorance with regard to the time-order of the reports where there was a special observational task, whether corresponding or non-corresponding.

We have data, however, from our own experiments bearing on the question of the influence of NST on ensuing processes of abstraction. For the first series these are given in Table III.

In order to obtain a comprehensive view of the somewhat irregular distribution of the orders of goodness these orders were added together for NST as observational task, for all cases where observational task and question corresponded, and for CP, LP, NL and F as observational tasks, but with the questions not-corresponding. Each of these six totals was then divided by the number of orders of goodness used in getting the particular total. Since the result of this division will be smaller, the higher the order of goodness is on the whole for the particular case, it is possible in this way to obtain a rough indication of how well the questions were answered in the different cases. The result is given in Table IV.

TABLE IV

Observ. task Question	CP LP NL F correspond.	NST CP LP NL F	CP LP NL F	LP CP NL F	NL CP LP F	F CP LP NL
Average order of goodness	2.19	2.9	3.58	3	3.46	3.08
Total order of goodness	1	2	6	3	5	4

According to the last line of Table IV, then, the observational instruction NST stands second only to the case where observational task and question correspond with regard to favourableness of its influence on the correctness of the answers given. It is clear that this result stands in contradiction with Külpe's statement as to NST being relatively unfavourable.

The irregularity of the distribution of the orders of goodness, as shown in Table IV, we hold to be due to the presence of a number of disturbing factors, which have been indicated already in part in a remark of Grünbaum, quoted previously, with regard to a "preoccupation of consciousness" for the characteristics of the object not specified in the observational task of a particular observation. It should be added, however, that the *Os* were asked to pay particular attention in their introspective reports, made after each single experiment, as to whether they had carried out the instruction of the observational task successfully or not. Only in quite a minority of cases was non-success reported. The disturbing factor would rather have taken the form then, we hold, of a predisposition, an unconscious mental set. In part these disturbing factors have still to be discussed. Series II was indeed largely arranged so as to avoid them. The main result as to the influence of the observational task NST, obtained as above in Series I, is however in full agreement with the result obtained in Series II, and its adduction may be justified because of its confirmatory value.

As shown by Table II, there were certain exceptions to Külpe's main contention with regard to the favourable influence of the positive abstraction of a characteristic where observational task and question corresponded, namely, in the cases of NL and F. That these exceptions should be more than apparent seems very unlikely in view of the law that "the stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect. Or, in popular terms, the object of attention comes to consciousness more quickly than the objects that we are not attending to,"[24]—at least if we can apply this law, or at any rate extend it, to the dependent as well as the independent parts of a complex object such as was used in our experiments. Such parts would then, especially in view of the comparatively short time of exposition, be able to attain to fuller consciousness than others. It is true, of course, as Grünbaum's already quoted remark shows, that a mental set for other characteristics or parts of the object than the one to which the observational task in a particular case refers is bound to develop in time; but still one would expect the higher degree of preoccupation with the characteristic corresponding to the observational task concerned to show its influence, unless disturbing factors prevent it from doing so.

As a matter of fact indeed the different observational tasks and questions, which we, following Külpe, gave our *O*s, are not equally suitable for the aim of the experiment; and it appears to us that this unsuitability becomes a serious cause of error in the case of NL.

Külpe himself remarks: "Um zu verhüten, dass sich eine bestimmte Vorstellung über den gleichartigen Aufbau der Objekte ausbildete, wurden von Zeit zu Zeit Vexierversuche eingeschoben, bei denen mehr oder

[24] E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 251. With regard to the application of this law, which is given as one of attention, to the case of abstraction, we have to refer first to Külpe's statement: "Man versteht im allgemeinen unter der Abstraktion den Prozess durch den es gelingt, einzelne Teilinhalte des Bewusstseins hervorzuheben und andere zurücktreten zu lassen. Von diesen letzteren, sagt man, wird abstrahiert, abgesehen, sie werden in Abzug gebracht, sie gelangen nicht zur Geltung im Bewusstsein. Die erfassten Teilinhalte dagegen *werden* abgelöst, abstrahiert, aus ihrer Verbindung, ihrem Zusammengegebensein mit anderen herausgehoben" (*op. cit.*, 56). We have to refer, secondly, to Rubin's remark: "Sein Schüler Grünbaum fügt hinzu (und was er sagt dürfte wohl mit den Anschauungen Külpe's übereinstimmen): 'Solche Absonderung kann . . .an unselbständigen Teilinhalten *eines* Objektes, an *gleichen* Merkmalen mehrerer Objekte, an selbständigen Teilinhalten eines Gesamtinhaltes, insofern sie durch diese oder jene Relation verbunden sind, vorgenommen werden' (Grünbaum, *op. cit.*, 343). Wenn Külpe statt Abstraktion Aufmerksamkeit and statt abstrahiert bemerkt geschrieben hätte, würde er in dem oben Zitierten eine Definition der Aufmerksamkeit gegeben haben, der mancher Psychologe beitreten würde" (*op. cit.*, 385).

weniger Silben bezw. mehr oder weniger Buchstaben in einer Silbe zur Verwendung kamen. Immerhin ist die Aufgabe *Zahl* durch das regelmässige Auftreten von 4 Silben à 3 Buchstaben und durch die über die Möglichkeit einer unmittelbaren Zahlbestimmung hinausgehende Zahl von 12 Elementen etwas beeinträchtigt worden."[25]

From the introspective reports of our *O*s it is clear that, notwithstanding similar blank-experiments to those Külpe introduced, they came to learn that in most cases there were four groups of three letters in each object, and that this knowledge was of assistance in answering the question as to NL. A factor such as this would tend, in an undesirable way, to equalize the scores with regard to corresponding and non-corresponding observational tasks and questions. Chance factors, such as unsuccessful carrying out of the observational task, fluctuations of attention in answering, or differences in the difficulty of the objects, may then, in a limited number of experiments, even bring about a somewhat higher percentage of correct answers in the case of a non-correspondence of observational task and question than in the case of their correspondence. In the second series we therefore omitted NL as observational task and question.

It is furthermore clear that, in the case of two of the observational tasks and questions, the *O*s had to carry out a double process of abstraction, either at the exposition or afterwards, in answering the questions, as far as the *content* is concerned, namely, in the case of *colours with positions* and *letters with positions*. Külpe does not state how he arrived at his points for correct answers with a view to the heterogeneous constituents of these answers. Our procedure, given above, of course means that points for heterogeneous contents were simply added together in arriving at the totals. It seemed desirable to avoid this in carrying out the second series of experiments, and Series II was accordingly so arranged that this could be done.

Finally, the answers given to the preceding questions, after an exposition, certainly influenced the answering of the succeeding ones, and this in a way that one is not able to control. Thus CP and LP take relatively long to answer usually, so that the time between exposition and answering further questions is increased where LP or CP preceded. In the same way, the intervening time is shortened when NL or F precedes. Again, CP having been answered, F can then be answered on the basis of the question already answered, whereas the opposite is not the case, at any rate to the same degree. The further change was therefore made in our second series that only one question was answered after each exposition.

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[25] *Op. cit.*, 58.

Series II

The second series was carried out with 12 objects, similar to, but other than those used in Series I. Time and means of exposition were the same as in Series I. The second series was divided into 4 part-series in each of which each of the 12 objects was exposed in succession to each of the 10 *O*s, thus making 480 observations in all.

The *O*s were the 4 who had taken part in the first series and Messrs. P. F. Greyling (Gr), J. G. Schoeman (Sc), T. F. Cronje (Cr), postgraduate students, and J. A. Oberholster (O), J. Mommsen (Mo) and P. Stander (St), graduate students of the University of Stellenbosch.

Preliminary practice was given the new *O*s with objects other than those used for the actual experiments. The observational instructions, one of which was, as in the first series, given before each exposition, were: (1) note the figure, (2) note the colours, (3) note the letters, and (4) the fifth observational task of Series I, namely NST. The questions referred to figure, colours and letters. The order of the observational tasks and the questions was so arranged as to make the impression on the *O*s of being a "chance" one. In each part-series observational task and question corresponded in 6 out of the 12 cases, each type being equally often represented in each part-series. The same observational task was never given twice with the same object, and two expositions of the same object were separated from each other by 12 expositions of other objects. Since there were 4 expositions and 4 reports on each object, the possibility arose, notwithstanding the separation of expositions just mentioned, of previous reports and observations, as carried out under the influence of the particular observational task set, affecting the percentage of correct answers in later cases in an undesirably favourable way, in so far as there was correspondence between the characteristics specially to be noted, according to observational task and question, in the case of the previous exposition(s) and the later expositions of the same object. Thus if, in the first series, the observational task was C (colours) with regard to a particular object and the question also referred to C, a later question C, with the same object but with a non-corresponding observational task, might show a result which benefited from the previous exposure and answer. To meet this, another pair of observations were then made with another object with C as question and a non-corresponding observational task coming in a similarly preceding part-series, and C as observational task and question with the same object coming in the succeeding part-series, and so on. In no other case was the same question repeated with the same object. Table V gives the quantitative results in percentages of correct answers for Series II.

TABLE V

Observ. task	C	NST	F	L	L	NST	F	C	F	NST	C	L
Question	C	C	C	C	L	L	L	L	F	F	F	F
Os												
M	68.7 (3)	81.3 (1)	62.5 (4)	75 (2)	48 (1½)	48 (1½)	39.6 (3)	—	88 (2)	100 (1)	—	74 (3)
Mo	93.7 (1)	68.7 (2)	62.5 (3½)	62.5 (3½)	37.5 (1)	35.4 (2)	31.2 (3)	—	74 (3)	100 (1½)	—	100 (1½)
O	59.4 (3)	68.7 (1)	62.5 (2)	50 (4)	19.7 (1½)	19.7 (1½)	16.7 (3)	—	88 (3)	100 (1½)	—	100 (1½)
St	87.5 (1)	81.3 (2)	62.5 (4)	75 (3)	36.5 (2)	35.4 (3)	37.5 (1)	—	100 (1½)	100 (1½)	—	74 (3)
V	84.4 (3)	75 (4)	100 (1)	87.7 (2)	42.7 (1)	40.7 (2½)	40.7 (2½)	—	100 (2)	100 (2)	—	100 (2)
P	68.7 (2½)	68.7 (2½)	50 (4)	75 (1)	29.2 (1)	20.3 (2)	16.7 (3)	—	100 (1)	74 (1½)	—	74 (1½)
Sc	68.7 (1)	43.8 (3½)	43.8 (3½)	62.5 (2)	14.6 (3)	19.7 (1)	16.7 (2)	—	88 (2)	74 (3)	—	100 (1)
Cr	62.5 (1)	56.3 (2)	50 (3)	25 (4)	19.7 (1)	14.6 (2½)	14.6 (2½)	—	74 (2½)	74 (2½)	—	100 (1)
C	81.3 (2)	93.7 (1)	62.5 (3½)	62.5 (3½)	37.5 (3)	39.6 (2)	40.7 (1)	—	100 (2)	100 (2)	—	100 (2)
Gr	59.4 (1)	56.3 (2)	50 (3½)	50 (3½)	16.7 (1½)	16.7 (1½)	14.6 (3)	—	100 (2)	100 (2)	—	100 (2)

The data given in this Table may be assembled further in different ways so as to bring out their bearing clearly on the points at issue; namely, either by working out the average percentage for the *Os* for each separate question for the two cases when (*a*) question and observational task correspond and (*b*) when they do not correspond; or by making use of the method of orders of goodness already previously applied. We content ourselves with giving the result if the second method be used, though it may be added that the results of using the first mathematical treatment are in principle in agreement with those adduced. No observations corresponding to the blank columns were made.

It is clear from the Tables that there is a decided tendency for the answers to be correct to a higher degree where the observational tasks and questions correspond than where they do

TABLE VI

Observ. task	C	NST	F	L	L	NST	F	F	NST	L
Question	C	C	C	C	L	L	L	F	F	F
Average order of goodness	1.85	2.1	3.2	2.85	1.65	1.95	2.4	2.1	1.95	1.95
Total order of goodness	1	2	4	3	1	2	3	3	1½	1½

not,—if we omit from consideration for the time being the case of F, which gives a somewhat similar result as in the case of Series I. It may be mentioned, however, that the distribution of the orders of goodness shows a certain amount of irregularity here too. This we hold to be largely due to the fact that the factor of a preoccupation or predisposition for the characteristics of the object not indicated in the observational task, or present in spite of it, had not been eliminated in the second series. In the long run these disturbances evidently cancel each other; so that, at any rate for C and L, the favourable influence of the observational task corresponding to the question comes out sufficiently clearly in Table VI.

Coming to the question as to the results reached in the case of F as observational task, namely that in the case of the first series only an uncertainly favourable, and in the case of the second series a definitely unfavourable result is obtained, as far as the figures are concerned, where observational task and question correspond, compared with the results where they do not correspond, we hold that they are to be explained on the supposition, generally stated, that the advantage accruing to answers relating to F, where F is also observational task, becomes relatively small as compared with the influence of other factors that also come into play.

It is clear that the mere fact of a mental set (or of a preoccupation of consciousness) for the characteristics of the object other than those indicated by the observational task actually set will not explain this difference between the cases of F on the one hand and those of C and L on the other, if it is held, as we hold, that this factor is present in all three cases; unless there is reason to accept the view that it plays a larger rôle in the case of F than in the others. It is possible that there was present with most, if not all the *Os* a relatively strong mental set for forms.

Seifert, working on the problem of abstraction by means of tachistoscopic experiments with objects consisting of points and small figures arranged so as to form a "Gestalt," adduces introspective evidence from his *Os* for the view that the "Gestalteinheit" especially draws attention. "In allen diesen Aussagen kommt eine wichtige Tatsache zum Ausdruck: Es scheint, als ob die die Empfindungsgrundlage überbauenden Gestalteinheiten als *Faktoren für die unwillkürliche Aufmerksamkeit* wesentlich in Betracht kommen . . . Es bleibt noch übrig, zu begründen, *warum* von der Gestalt jener Zwang zur Beachtung ausgeht, warum die 'Auffälligkeit' der *Gestalt* grösser ist als die der Fundamente. Das Erklärungsprinzip bildet das . . . Moment des 'Gestaltungsdrangs'. In dem Sinn dieses Begriffs liegt ja das Moment der konstanten *Bereitschaft zur Gestaltbildung*. Man könnte hier mit vollem Recht von einer unbewussten Einstellung reden."[35]

[35] F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 107-108.

Clearly, then, a factor such as this could possibly have had a greater influence in the case of F than in the case of C and L (and CL and LP) in equalizing the scores for the cases of correspondence and non-correspondence. There is, however, another factor present which would act systematically in the same direction, and thus make it possible for chance factors such as fluctuations of attention in holding the observational instructions in consciousness, in observing the object and in answering the questions, as well as differences in the difficulty of the figures, to give a result, in a limited number of experiments, which does not agree with the influence the correspondence of observational task and question may be expected to exert.

In coming to the discussion of this further factor it is worth while taking into account the fact that the scores in the case of F as question are relatively high. This is seen at once on examination of Tables I, II, III and V, the results of the first and second series agreeing also on this point. As Külpe's Tables show, the same thing holds true for his results. Merely to give as an explanation for this that the answer to F is easier is of course unsatisfactory, as long as that is only another way of stating that the scores are higher.

The figures occurring in our experiments were relatively simple unitary forms, being dependent abstract characteristics of the complex concrete object. When the *O*s set themselves to answer the questions set after the exposition they have to carry out the second act of abstraction, chronologically considered, that was mentioned above. This act of abstraction is carried out on the basis of the dispositional after-effects of the observation of the object at exposition and possibly of a process of abstraction which has taken place directly in connection with it as result of the observational task. The second act of abstraction is thus a recollective one. Now Selz has found with subject-matter of a totally different kind from that used here, but where the abstraction had also to be carried out on the basis of memory after-effects, that there is an autogenous abstractive preparedness for the more general and especially for the most general characteristics of the subject-matter.[27]

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[27] O. Selz, *Psychologie des produktiven Denkens und des Irrtums*, 1922, 116. "Wir müssen . . annehmen, dass der *logischen Struktur* der Begriffe aus allgemeineren (abstrakteren) und spezielleren (konkreteren) Merkmalen Eigentümlichkeiten in der *psychologischen* Struktur der Bedeutungskomplexe und der entsprechenden Komplexdispositionen entsprechen, welche die selbständige Reproduzierbarkeit der allgemeinsten Merkmale möglich machen. Die beobachteten Tatsachen führen ferner zu der Annahme, dass in den Bedeutungskomplexen den allgemeineren und spezielleren Charakteristiken *Schichten von verschieden hohem Bereitschaftsgrad* entsprechen, und dass hierbei die allgemeinsten Charakteristika der gemeinten Gegenstände eine Schicht von höchstem Bereitschaftsgrad

Certain errors and omissions made by Grünbaum's *Os* in drawing the figures, after exposition, which had been shown them, offer further evidence in support of the view just stated. So, *e. g.*, details are forgotten, but the figure is correctly drawn schematically; or the *O* can give only a part, which is however often characteristic of the figure; or the position of parts is transposed (Spiegelzeichnung). *Op. cit.*, 391. In all these cases abstract characteristics are more easily abstractively recollected. According to Rubin[28] Moore had observed similar phenomena in his experiments; and, as I understand Rubin, had found that the assimilation of the figure under some more general mental category took place so that this more abstract characteristic was often remembered afterwards, whereas more concrete details could no longer be remembered. The process of recollective abstraction would in such cases benefit from a preceding process of abstraction.

In part this autogenous abstractive preparedness of the more abstract characteristics of an object will be the result, in some cases at least, of previous processes of abstraction which were carried out with regard to the same characteristics to which the second act of recollective abstraction is directed. In part, however, it will be due to the fact that these abstract characteristics are, as abstract, relatively simple, the simplicity increasing with the degree of abstractness; so that, other things being equal, they could more easily be brought to consciousness on the basis of the dispositional after-effects than the fuller and more concrete contents. The figure is such a more simple content in the case of Külpe's and our objects, forming, logically considered, one of the abstract characteristics of the object. For this reason a process of recollective abstraction can be carried out more easily with regard to it, even when no corresponding abstractive process preceded. We have, then, here a further factor tending to equalize the scores where observational task and question correspond and where they do not.

Finally, we return to a point raised in connection with the first series, namely the influence of NST on the answering of the questions. Table VI, as derived from Table V, shows that also in the second series the questions were answered relatively well in this case, a result which, as has been pointed out before, is not in agreement with Külpe's statement.

It is possible that the results which Külpe has tabulated, in so far as some do actually show a poor score with the instruction "ohne Aufgabe," are due in part to a difference from our NST instructions in that somewhat more emphasis is laid on the passivity of the *Os* in the case of Külpe's instructions. Partly, too, in his case some of the reports were given only

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bilden." He points out that this agrees with results of Messer and Bühler and adds (501): "Infolge der hohen autogenen Abstraktionsbereitschaft der allgemeinsten Charakteristika werden diese auch hervortreten, wenn die Determination nicht speziell auf das Hervortreten *irgendwelcher* Charakteristika gerichtet ist," etc.

[28] *Op. cit.*, 395. T. V. Moore's own article (The Process of Abstraction, *University of California Public. in Psychology*, I, 1910) was unfortunately not at my disposal.

after the reports on points which had specially interested the *Os* had already been made. Speculation with regard to the influence of the order in which the questions were set becomes somewhat uncertain, however, in view of the paucity of information Külpe gives on this point.

On general principles one would expect the *Os* to get better results with the observational instructions NST where they *really attend* to the figure as a whole when exposed and where disturbing factors do not play too great a rôle, than where they attend especially to particular characteristics or constituents and have then to answer questions as to others. In the first case there would be a relatively more even distribution of attention over the different constituents and characteristics, so that the dispositional after-effects, on the basis of which the later process of recollective abstraction would have to take place, would tend to be more favourable for this process than where weaker dispositional after-effects for some had been left, as the result of focussing attention on others.[29]

So Moore[30] found that with the perception of an abstracted element the remembrance and perception of other elements were strongly disadvantageously affected. Seifert found that "zwischen einer abstrahierenden und einer (gleichzeitig aktualisierten) gestaltbildenden Tendenz besteht ein eindeutig bestimmtes Korrelationsverhältnis; es scheint, als ob die beiden Momente sich einander kompensierten. Gelingt die Gestaltauffassung gut, dann wird die Abstraktionsaufgabe beeinträchtigt; wird dagegen die Abstraktionsaufgabe vollkommen gelöst, so bleibt die Bildung der Gestalt unvollkommen."[31] The "Gestaltbildung" is, of course, in our terminology, the abstraction of a particular characteristic of the object Seifert showed his *Os*. Grünbaum, while getting similar results on the whole with regard to the effect of fulfilling the "Hauptaufgabe" on the fulfilling of the "Nebenaufgabe," found in certain cases that "die maximale Anspannung auf die Lösing der Hauptaufgabe . . . auch der Nebenaufgabe zugute kommt," where the chief task was the finding of identical figures in two groups of figures and the subsidiary task that of drawing or recognising the figures which were not identical. These were cases in which there was a large number of figures in the groups, thus rendering the carrying out of the chief task more difficult. He finds thus that "die Anspannung auf die Hauptleistung, die Konzentration des Bewusstseins auf sie, der Nebenleistung zugute kommt" and points out that distractions of attention sometimes lead to an improvement of the process actually to be carried out as task, although it is still a problem how this transfer is really fully to be explained,[32]—a statement which

[29]We have to leave it an open question in how far, in the case of our *Os*, an actual process of determined negative abstraction took place with regard to that which was not given in a special observational task, and in how far it simply attained to a lower degree of consciousness or did not become conscious at all. With regard to this difference between negative abstraction and non-observance see Grünbaum, Negative Abstraktion und Nebenaufgabe, *Arch. f. d. ges. Psychol.*, 38, 1919, 173-174.

[30]According to R. Pintner, *Arch. f. d. ges. Psychol.*, 26, 1913, 55 (Literaturbericht).

[31]*Op. cit.*, 109. Cp also E. Achenbach, Experimentalstudie über Abstraktion und Begriffsbildung, *Arch. f. d. ges. Psychol.*, 35, 1916, 535-6.

[32]*Op. cit.*, 411-413.

still holds true after Westphal's work, in which he sets himself the problem of examining the relations between the carrying out of the chief and subsidiary tasks. Westphal finds, however, that "je höher. . .die Stufe ist, auf der das Resultat der einen Aufgabe gegenwärtig ist, um so weniger ist über die andere zu sagen."[33]

Accepting that, under the influence of the consciousness of a task as being more difficult than ordinarily is the case, a spurt of attention may take place, and that, whatever the further explanation may be, a "transfer," affecting a subsidiary task favourably, such as Grünbaum found, may take place, we should not be in contradiction with our result with regard to the relatively favourable influence of the observational task NST, as long as we hold, as we do, that, on the whole, our *O*s were as attentive, at least approximately, in the case of this task as in the case of other more particularised ones.

Summary

(*a*) As the result of an error in procedure Külpe's tachistoscopic experiments on abstraction are shown not to have proved that the percentage of correct reports is higher in the case where observational task and report correspond than in the case where they do not correspond.

(*b*) If the experiments are carried out with regard to a subject-matter which does not introduce disturbing factors to a sufficiently high degree and in such a way as to eliminate Külpe's error, it is found that Külpe's result, as just stated, is still true in principle.

(*c*) Over against Külpe's statement that observations "ohne Aufgabe" are on the whole disadvantageous for abstraction, it is found that, when the observation is carried out attentively but with "no special observational task set," the results with regard to the correctness of the reports are more favourable, with suitable subject-matter, than where the observational task and question are directed to different constituents or characteristics of the object exposed tachistosocopically; but that the results are then less favourable than where they do correspond.

[33]E. Westphal, Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.* 21, 1911, 405.

THE PSYCHOLOGY OF *GESTALT*[1]

By HARRY HELSON, Cornell University

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(1) *Introductory*

Among the various schools of psychology there has arisen rather suddenly in Germany a new school with a definite set of assumptions and theories of its own which has come to be known as *Gestalttheorie* or configurationism.[2] The new school may be said to date from the publication in 1912 of Wertheimer's article on seen movement, where the main theses of configurationism first find clear expression.[3] Since then the fundamental concepts of configurationism have been adopted by others, notably by Koffka and Köhler, and a large literature bears witness both to the work which these men and their followers are doing and to the discussion, *pro* and *con*, which the new theories have aroused. It is not surprising that the theories of the configurationists have attracted widespread attention both in Germany and abroad, for the leaders of the new school have applied their concepts to practically every field of interest in psychology; beginning with perception, their investigations have extended into the fields of learning, memory, thought and action in human psychology, in animal psychology, and in the

[1]The material contained in this and the following articles was first presented, in modified form, as a thesis at Harvard University, in partial fulfillment of the requirement for the degree of Ph. D., April, 1924. I am indebted to Professor Titchener for suggestions and criticisms.

[2]As a translation of the word *Gestalt* I have used the word configuration, proposed by Professor E. B. Titchener. Various grammatical forms of the word are often useful, and will be understood from the context.

[3]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psych.*, 61, 1912, 161-278.

psychology of the abnormal. In fact, the proponents of the configurational theories assert that their concepts apply to physical and biological problems as well as to purely psychological questions, with the result that they have advanced new physical and physiological hypotheses to support their psychological views.

In its beginnings the new psychology has inevitably been destructive; for it has arisen as a protest against the assumptions underlying the methods and hypotheses of older schools. It has brought together from different quarters the criticisms levelled against the traditional psychologies, and it utilizes them in a frontal attack on association, attention, sensation, conditioned reflex, trial and error, and a whole host of concepts which inform current psychological systems. And it has also resurrected many historical concepts which have either fallen into the discard or play but a minor rôle in current psychological theory: the concept of the *Gestaltqualität* finds a new interpretation and becomes the basis of a new theory; act-psychology contributes its share, in the demand that mental life shall be conceived as a dynamic functional process for which functional concepts must be employed in description and explanation; phenomenology prompts an adherence to observable processes and entities and the rejection of results of artificial laboratory methods and products of subsequent reflection, be they unconscious acts, acts of attention, hypothetical sensory elements, or unobservable associative mechanisms.[4] After attacking certain older psychological concepts, the configurationists thus proceed to the erection of the structure which they wish themselves to substitute in place of the old.

In this article I seek to portray the main points of departure in configurational theory and the direction in which they seem to lead. In two following articles I shall present the experimental work upon which the new theories are based, along with the work of members of other schools in similar fields. And in a fourth article I shall attempt a critical estimate of what the school has accomplished and what it seems to offer to the psychology of the future.

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[4]The connection between configurationism and the school of the form-quality is close, as will be seen from this article. Many of the problems concerning the form-quality reappear in the configurational theories. For an excellent summary and discussion of the whole movement, see this JOURNAL, Psychology of Mental Arrangement by M. Bentley, 13, 1902, 268-293. For act-psychology, see A. Messer, Ueber den Begriff des Aktes, *Arch. f. d. ges. Psych.*, 24, 1912, 245ff., and E. B. Titchener, Functional Psychology and Psychology of Act, this JOURNAL, 33, 1922, 43-83. For the influence of phenomenology on psychology *cf.* A. Messer, Husserl's Phänomenologie in ihrem Verhältnis zur Psychologie, *Arch. f. d. ges. Psych.*, 22, 1911, 117-129, and *ibid.*, 32, 1914, 52-67.

The configurational revolt is not an isolated, unique phenomenon in the history of modern psychology. The school of the form-quality endeavored to account for perceptual complexes and "objects of higher order" by introducing the notion of a new element added to the usual list; but in the hands of Meinong the theory became increasingly logical, and finally ended in epistemology. The new elements of the Würzburg school were just as much a protest against the Wundtian psychology of sensation, feeling and apperception as they were an attempt to deal with the thought processes; yet nothing has come of this movement. Act-psychology and its first cousins, functional and behavioristic psychology, seek to explain the activities of the organism in long-section, decrying the cross-sectional analyses of the existential and process psychologies. There have, moreover, been other influences at work to foment a new movement in Germany. The most important of these is phenomenology, which has given rise to a new "descriptive" psychology, closely related to the *Geisteswissenschaften*, whose object it is to approximate the facts of everyday life more closely than experimental psychology has done. Here the influence of Dilthey and Husserl has been felt, especially on the side of method. Husserl[5] in 1910 insisted that psychology should not try to ape the physical sciences by copying their methods, since their analyses do not lend themselves to the subject-matter of psychology; and Dilthey[6] as far back as 1894 had stated that psychology should employ concepts peculiar to itself and adequate to its own problems. At the same time there were those in experimental psychology itself who attempted to deal with complexes and relations from new points of view, though so far with little success in achieving a new system. In this group the followers of Meinong stand out as the natural inheritors of the tradition of the form-quality, a fact which makes them close kin of the configurationists. In fact, Benussi, Witasek and Höfler dropped the word "quality" and spoke of "form" (*Gestalt*) before Wertheimer's paper appeared in 1912.[7] Dissatisfaction with traditional physiological theories, built upon atomistic-associa-

[5]E. Husserl, Philosophie als strenge Wissenschaft, *Logos*, 1, 1910, 309.

[6]W. Dilthey, Ideen über eine beschreibende und zergliedernde Psychologie, *Sitzungsberichte der königl. preuss. Akad. der Wiss. zu Berlin*, 1894, 11.

[7]Meinong had referred to complexes as *Gestalten* as early as his 1894 paper, Beiträge zur Theorie der psychischen Analyse, *Z. f. Psych*, 6, 1893-4, 340ff. *Cf.* also *Untersuchungen zur Gegenstandstheorie und Psychologie* 1904, edited by Meinong; S. Witasek, *Grundlinien der Psychologie*, 1908, and *Psychologie der Raumwahrnehmung des Auges*, 1910; V. Benussi's list of papers is large, but for a summary of his views see Gesetze der inadäquaten Gestaltauffassung, *Arch. f. d. ges. Psych.*, 32, 1914, 396-419; A. Höfler, Gestalt und Beziehung—Gestalt und Anschauung, *Z. f. Psych.*, 60, 1911-12, 161-228.

tionistic postulates, had already been expressed as early as 1898 by von Kries and in 1911 by Becher, who asserted that current theories could not explain the perception of form, relations, magnitude, similarity, general ideas and the thought processes.[8]

The stage was therefore set for the new movement in 1912. Instead of reconstructing or adding to the older theories, as most of their predecessors had done in their criticisms of traditional concepts, the configurationists insist upon starting out afresh, demolishing the old systems, and building anew from the foundation up. It will therefore be necessary to set forth the configurationists' objections to certain traditional assumptions and concepts before I attempt to state their positive programme of reform.

(2) *The Rejection of Analytic Methods*

The method of analysis, which is the chief instrument of scientific psychology, has been the main object of the configurationists' criticisms.[9] Wundt, Watson and leaders of other movements assume, the configurationists assert, that a direct analysis of mental life or behavior is possible. Direct analysis appears to them to be comparable to the actual dissections of the histologist, the partitions of the physicist or chemist. But the configurationists are convinced that the ultimate sensational and behavior units are a product of ratiocination and reflection or of an artificial 'laboratory attitude' toward the facts. Objects, meanings, values and dynamic processes are thus analyzed into meaningless units which have nothing to do with the patterns in which they originally appeared. Very often configurational structures have been excluded from the scientific field of psychological investigation. The configurationists deny that Müller's reproductive tendencies, the acts of the act-school, the chain-reflexes of the behaviorists, or the laws of association can explain how the postulated units form complex structures, how they come to mean. And a further objection against analysis touches the fact that it is impossible to synthetize what has been taken apart, unless additional assumptions are introduced *ad hoc* to explain the original totality: they cite as such assumptions Helmholtz' theory of unconscious judgments to explain contrast in vision, Wundt's principle of creative resultants to account for new emergents in perception, and J. S. Mill's doctrine of mental chemistry to supplement association.

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[8] J. von Kries, *Ueber die materiellen Grundlagen der Bewusstseinserscheinungen*, Program der Universität Freiburg i. B., 1898; and E. Becher, *Gehirn und Seele*, 1911.

[9] *Cf.* M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psych. Forsch.*, 1, 1922, 47-58; 4, 1923, 301-350.

The assumption that parts constitute wholes must, they maintain, be rejected, if we are to remain true to the facts of observation. For wholes are the first data given in perception, both phylogenetically and ontogenetically. The ape, Köhler found,[10] reacts more quickly to a situation which he can survey as a whole (structure-function reaction) than to absolute units; Koffka believes that the child learns to recognize and react to the "friendly" face before it has any perception of its colors, etc., as such.[11] The evidence from the field of the psychology of the abnormal has been differently interpreted by Fuchs, Gelb and Goldstein on configurational principles and by Poppelreuter on others.[12] I may quote Fuchs at this point in illustration of the configurational position: if part of a circle is so presented tachistoscopically to a patient suffering from hemianopsia that the incomplete part of the figure falls within the blind portion of the retina, the patient will see a complete circle. The completion is due to the configuration. The elements of analysis are therefore not the real data of experience: elements, sensations, parts are the result of an artificial laboratory attitude which causes the downfall of the originally given structure. When the observer assumes an analytic attitude, the object of observation changes accordingly from a "natural" datum to another which is less natural, more poorly structured. On this point Koffka has taken issue with those psychologists who believe that change in attention or attitude leaves the object unchanged except for differences of clearness. Thus the perception of a field now as figure and again as ground means that a new object is present with new properties, not that the field has merely changed in clearness. So, too, with the staircase illusion: the inverted figure possesses new phenomenal properties, to which a mere change in clearness does not do justice.[13]

The problem of attitude still remains a difficult one for the configurationists, in view of the fact that their own results go to show that "analytic" attitudes may be as natural to some observers as the "synthetic" attitude is to others. Benussi, Seifert, Linke, Kenkel, Gneisse and Wulf have noted

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[10]W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, Nr. 2, *Abh. d. konigl. preuss. Akad. d. Wiss.*, 1918, Phys.-Math. Klasse.

[11]K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921. Trans. by R. M. Ogden, *The Growth of the Mind*, 1924.

[12]W. Fuchs, Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker, 1, *Z. f. Psych.*, 84, 1920, 67-169; and 86, 1921, 1-144; A. Gelb and K. Goldstein, Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges, *Z. f. d. ges. Neurol. u. Psychiat.*, 41, 142; W. Poppelreuter, *ibid.*, 83, 1923, 26-152.

[13]K. Koffka, Perception: An Introduction to *Gestaltheorie*, *Psych. Bull.*, 19, 1922, 531-584.

this fact.[14] To these findings Koffka retorts that the analytic attitude results in the formation of a new configuration, different from the configuration found in the natural state.

The other objections against analysis concern the unity and specificity of structures. If configurational structures possess specific properties which are supra-summative and belong to them as wholes, then these properties are lost in analysis. In other words, supra-summative properties "are not compoundable from similar properties and activities of so-called parts". And, following Ehrenfels,[15] Köhler says: "one finds space-forms which possess more properties than their elementary visual sensations; the same is true of melodies with respect to the tones which compose them, and of intellectual processes with respect to the data in which they originate. For it cannot be assumed that sensations of color and tone, and meanings of single words, are to be considered as 'parts' of space-forms, melodies and higher thought processes; since the exact impression of a visual figure or of the specific character of a musical motif, and the meaning of an intelligible proposition, contain more than a sum of patches of color, tone sensations, and individual word-meanings."[16] Configurations, Wertheimer insists, must be conceived of as governed by internal laws, as opposed to the summative, contingent, spatio-temporal contiguities of meaningless elements. Only in this way can the whole be explained as a rational, meaningful structure.[17]

The arguments so far advanced against analysis are based upon its effect upon the internal structure of the datum analyzed. From another point of view the configurationists find analysis of the direct kind unsatisfactory,—in that it does not satisfy certain demands which we make upon any scientific method: we must be able to control and predict what will happen to our data. It is impossible to describe or derive real wholes from a knowledge of parts. Since the whole possesses its own specific properties, we can never tell in advance from a knowledge of the parts what the whole will be or how it will behave. In this connection Fuchs has some evidence: patients suffering from hemianopsia tend to displace objects as wholes which may develop new centers of clearness varying with the size of the

[14]Benussi, *op. cit.*; F. Seifert, Zur Psychologie der Abstraktion und Gestaltauffassung, *Z. f. Psych.*, 78, 1917, 54-144; Kenkel and F. Wulf in Koffka's *Beiträge*; P. Linke, *Grundfragen der Wahrnehmungslehre*, 1913; K. Gneisse, Die Entstehung der Gestaltvorstellungen, *etc.*, *Arch. f. d. ges. Psych.*, 42, 1922, 295-334.

[15]C. von Ehrenfels, Über Gestaltqualitäten, *Vjs. f. wiss. Philos.*, 14, 1890, 249ff.; 15, 285ff.

[16]W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, ix. This is a free and adapted translation.

[17]Wertheimer, *ibid.*

object (pseudofovea).[18] An investigation of the retina point by point would never have enabled us to predict these new centers of clearness. In place of direct analysis the configurationists substitute what Koffka has called functional analyses: the method of functional analysis consists in limiting the stimulus instead of changing the phenomenal datum. Variations in the stimulus-situation can thus be controlled and measured. Functional and configurational situations may then be regarded as identical; for whenever one perceives or reacts to a whole differently from what one would to the parts in isolation, there we may say are observable differences which must be accounted for both descriptively and functionally. At the same time the descriptive and functional aspects of experience are subsumed to the same concepts and fit into a single conceptual system.[19]

It is noteworthy that the configurationists discourage analysis even of spatio-temporal wholes. It has long been recognized that the analysis of qualities offers difficulties; but spatio-temporal contiguity has been regarded as a connection which does not change the parts entering into the union. The configurationists believe that space-time may make a difference to units which enter into spatio-temporal wholes. For example, the form of a square is a function of the spatio-temporal relations, since changing these changes the configuration; yet there is something over and above the parts in relation once they have entered into relation. Spatio-temporal configurations are found in the domain of physics, according to Köhler, where two electric fields undergo a change in potential when brought near each other. Here a new configuration seems to appear as a result of the new spatio-temporal arrangement of the single fields.[20]

The decisive factors within any given configuration which are responsible for the new properties of the whole and for the modifications of the parts within the structure are varied and often elusive. Sometimes it seems to be the form of the group which is of especial importance in determining the character of the configurational changes; at other times it may be some single factor within the group which exerts its influence over the rest; or again it may be the internal relations of the parts which determine what the whole shall be. And often the configurationists confess that the determinants of configurational events are due to a variety of factors some of which are unknown. The Graz configurationists have here raised certain fundamental questions regarding the origin and nature of configurations. In the perception of square, for example, the configurationists deny that the relations of the lines to each other are responsible for the "squareness." Are then the new properties of the square as such a result of squareness, or does squareness set into operation some factor which is responsible for the emergents? The

[18] W. Fuchs, *opp. citt.*, but more especially, Eine Pseudofovea bei Hemianopikern, *Psych. Forsch.*, 1, 1921, 157-186.

[19] K. Koffka, Zur Theorie der Erlebniswahrnehmung, *Ann. d. Philos.*, 3, 1922, 375-399.

[20] Köhler, *ibid.*

Graz configurationists have answered such questions by postulating a special higher psychical activity (*Produktionsvorgang*) which stands over and above the sensory contents and orders them. Wertheimer, Koffka and Köhler, on the other hand, while they agree that there is something over and above the parts in relation, deny that special processes or acts are necessary to explain this fact: the new properties of structures, they say, belong to them as totalities, and are functions of the configuration itself.[21] But the Graz configurationists maintain that the configuration is by no means univocally determined, whether as to form or as to structure. Thus four dots arranged in the form of a square may be seen as two horizontal lines, two vertical lines, or two oblique lines at right angles to each other. Benussi therefore holds that the determinant in equivocal cases must be some higher psychical factor which unites sensory contents into patterns.[22]

The configurationists of the stricter school have replied to these and similar arguments. Bringing in new elements or special acts will not, they declare, explain the configurational phenomena. The parts of the configuration must be evolved from changes within the whole. Imageless thoughts, special acts, unconscious or unnoticed acts and sensations are all cast into the limbo of unobservables, all carry with them the assumption that mental life or behavior is composed of elements or units enjoying independent existence and united by summative bonds.[23]

The configurationists also attack the behavioristic and chance theories of learning formulated by Watson and Thorndike and their followers.[24] The main weakness of these theories, according to the configurationists, is that they are based on the assumption that intelligent behavior consists of part-activities which in themselves are meaningless. How these units come to be linked together in meaningful, purposeful behavior is left unexplained. Chance connections of reflexes can never account for correct solutions of problems presented for the first time. Chance theories also leave out of account the importance of the end or goal which gives significance to each act leading to its attainment. Still another difficulty of these theories is that behind them lurks an atomistic physiology: physiological units

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[21]For a full discussion of this point, see Koffka, Zur Grundlegung der Wahrnehmungspsychologie, eine Auseinandersetzung mit V. Benussi, *Z. f. Psych.*, 75, 1915, 11-90; and W. Köhler, *Physische Gestalten*. C. Spearman calls the Graz configurationists the "binarians" as opposed to the "unitarians," *Brit. J. Psych.*, 15, 1925, 211ff.

[22]V. Benussi, *op. cit.*

[23]M. Wertheimer, *op. cit.*

[24]K. Koffka, *The Growth of the Mind*; W. Köhler, *Intelligenzprüfungen an Menschenaffen*, 1921. Trans. by Ella Winter, *The Mentality of Apes*, 1925.

function in isolation from each other and independently of the whole organism; organic activities become summations of part-activities. The configurationists assert that, physiologically, the typical neural, muscular and glandular processes are not composed of single units corresponding to discrete aspects of the stimulus-situation, but rather must be regarded as total processes in which the correlations and correspondences are between total structures. These total structures determine, descriptively and functionally, the specific character of their parts.

The chance-reflex theories substitute muscular and glandular responses knit together by chance, or the pleasure principle, for the real behavior of the organism. Observation of behavior has given place to observation of units which have nothing to do with the original situation calling forth the conduct to be explained. The configurationists urge a return to *Gebaren*-observation instead of description in terms of analytic units.

Nor does the general programme of the behaviorists (if there are any thoroughgoing behaviorists left) meet with the approval of the configurationists, who insist that phenomenal patterns may be of the greatest influence in determining behavior patterns. To understand behavior we must understand the action of the environment upon the organism, and often only an introspective report can give us the information we need concerning the phenomenal side of experience.

(3) *Sensation and the 'Constancy' Hypothesis*

The sensation is closely allied with the current concepts of analysis, functioning in some systems as the last unit of analysis. Many of the facts which the configurationists have explained without resorting to sensation have received attention from members of other schools. But the explanations of the so-called atomistic psychologists do not satisfy the configurationists. Thus G. E. Müller has sought to account for the perception of groups or structures by means of traditional concepts. The perception of complexes, according to this writer, takes place by means of rapid shifts of attention from one member of the group to another. Müller postulates simultaneous and successive acts in which groups are perceived. As a result of his theory Müller comes to the conclusion that it is more difficult to grasp wholes than their parts, especially when one is fatigued, because configurations require collective perception and collective perception requires effort.[25] Koffka, taking issue with this position, asserts that it is more difficult and less "natural" to attend to the parts than to the whole: it is more difficult to perceive an overtone than the total clang, and a note requires more effort of

[25] G. E. Müller, *Komplextheorie und Gestalttheorie*, 1923.

perception than a melody. The more fatigued the observer, the more difficult it is for him to isolate natural parts from their context,[26] though not the artificial parts on which Müller's argument rests.

The sensation as the ultimate unit in psychophysics, perception and thought has given rise to what Köhler calls the 'constancy' hypothesis.[27] This hypothesis in turn is responsible for the use of unobserved processes and entities in order to explain why the constants in our systems deviate from what we have been led to expect of them. The constancy hypothesis implies that we can correlate simple aspects of the stimulus with simple ultimates in experience. When it is impossible to obtain a direct one-to-one correlation between sensation and stimulus, the nervous system furnishes the required units in the form of single excitations aroused by the stimulus and united by associative paths. Atomistic psychology thus goes hand in hand with an atomistic physiology. The elements, both physiological and psychological, count for one in a democracy of individuals, regardless of the context in which they appear. Perception, thought, memory, imagination, and the whole furniture of mind are regarded as summations of these simple units. The whole contains no more than a sum of its parts, the parts remain unaltered in the whole.

The associationists, when a given totality does not behave as they were led to expect from their knowledge of the parts, introduce such concepts as attention, apperception, tied images, and illusory or completion mechanisms to fill the gaps in our knowledge. These processes and mechanisms harnessed into explanation may or may not be observable. When they are not observable, it is plainly impossible either to test or to refute them. The doctrines of unconscious or false judgments, unnoticed sensations, special acts, and imaginal completions all spring from the same source, a desire to preserve the constant value which a constant stimulus is supposed to have for the organism under all conditions. But phenomenal fields may vary while the physical stimuli remain constant. To maintain that unobservable processes are responsible for the variations means, says Köhler, that the whole problem is withdrawn from observation; and then one *ad hoc* explanation is as good as another. The lack of one-to-one correspondence between stimulus and perception or response is due to the fact that other factors may be of equal or greater importance in the total situation than the stimuli; central processes may change the whole situation,

[26]K. Koffka, *Psychologie* in *Lehrbuch der Philosophie*, shortly to appear; Köhler has answered Müller at length in *Psych. Forsch.*, 6, 1925, 358-416.

[27]W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Z. f. Psych.*, 66, 1913, 51-80.

subjectively and objectively; an indeterminate *Aufgabe* may lead to almost any kind of report; and "attention" may change not a single sensation alone, but the whole state of the nervous system, with a concomitant change in the phenomenal configuration.[28]

There are no isolated, elementary experiences except under that most artificial analytic *Einstellung* which favors the disintegration of the structured perception. Configurations consist of at least two "parts" (figure-and-ground) entering into reciprocal relationship with each other. Any change occurring in one part of an experience affects the rest. On the behavioristic side, responses are made to total situations; they are "structure-function," "step-wise" reactions. What the configurationists are fighting becomes clear if we compare Köhler's explanation of the learning of apes with that of Jaensch, Schumann, and Lindworsky. Köhler trained a number of these animals to pick their food from the darker of two gray containers. When a third container, darker than that to which the animals had been reacting, was introduced, the animals picked the darker of the last two present, and not the one to which they had been reacting the most frequently, recently and vividly. The perception must, therefore, in order for transposition to take place, have included the two shades of gray within a unitary configuration. The reaction was to the situation "darker than," not to the parts as such. The animals can be taught to make absolute choices, Köhler tells us, but such learning takes longer, and lasts for a much shorter period, than the responses to total situations.

Jaensch,[29] following Schumann, explains the structure-function reactions by means of "transitional sensations". The theory of transitional sensations is based upon the hypothesis that over and above the primary sensory processes are other experiences which accrue to the primary. By means of these transitional states the animal compares the stimuli and can discriminate between them. Jaensch would explain Wertheimer's and Linke's phenomenal movement on similar grounds. Koffka, defending Köhler's theory, denies that transitional sensations can explain the transfer from one situation to another, since no addition of elements can account for sameness of structure. Once the configurational similarity is admitted, elements become superfluous. Furthermore, the transitional sensations are not observed; only the primary "sensations" are in consciousness at the moment the judgment is made. "Wir sehen im Farben-Paar ein Zueinander, eine Struktur, dazu brauchen wir kein Uebergangs-Erlebnis, wohl aber setzt das Uebergangs-Erlebnis die Existenz einer Struktur voraus."[30]

In so far as atomistic theories have tried to account for objects, meanings and values, the configurationists feel that their

[28] K. Koffka, *Theorie der Erlebniswahrnehmung.*

[29] E. R. Jaensch, *Einige allgemeine Fragen der Psychologie und Biologie des Denkens, erläutert an der Lehre von Vergleich.* Similar views are held by J. Lindworsky, Umrisskizze zu einer theoretischen Psychologie, *Z. f. Psych.*, 89, 1922, 313-357.

[30] K. Koffka, *The Growth of the Mind.*

explanations are inadequate. But they complain that ordinarily these terms find no place in scientific systems. The result has been that special disciplines, phenomenology and *Gegenstandstheorie*, have been founded to include them. With regard to the first point, Köhler complains that neither single elements nor any number of them can account for such meanings as symmetrical, triangular or object forms, whether we postulate simultaneous or successive acts of perception to unite the elements. To bring in tactual and other associative elements does not help, because the problem is only pushed back to other sense modalities. If predicates like outside, inside, round and the like will not apply to elements, they cannot apply to combinations of elements, unless we introduce further assumptions to supplement the older theories. "Aber ganz und gar nicht kann man aus ihr [der Assoziationslehre] ableiten weshalb das Tintenfass als *ein* optisches Gebilde, der Bleistift wieder als eines, das Buch wieder als eines, ein zufällig daliegender und irgendwie geformter schwarzer Stein auch als Einheit gesehen wird, weshalb überhaupt im Gesichtsfeld 'Sehdinge' von bestimmter Grösse, Begrenzung, Ausdehnung vorhanden sind."[31] The problem of perception is for the configurationists a problem of unity and structure, and not a problem of analysis. They wish to account for all the facts of perception by means of the simplest set of assumptions without bringing in supplementary aids for this or that particular anomaly of meaning or process. It is fallacious, Koffka argues, to infer that all experience is composed throughout of elementary processes. The sensation as a hypothetical unit had outlived its usefulness when psychology entered upon the investigation of dynamic phenomena, living processes and objective meanings.[32]

(4) *Associational and Relational Theories Examined*

It has not been a matter of chance in the history of psychological theory that the concepts of analysis, sensation and association have required and supplemented each other in their systematic setting. Once spatial perceptions have been analyzed into sensations, the course of ideas and thinking analyzed into a number of elementary processes, and recognition analyzed into a reappearance of contents conditioned by reproductive tendencies, some principle or mechanism must be postulated to explain how it came about that the original structure was a unity. The configurationists point out that association has been pressed into explanation in all manner of ways: it has been used to explain the facts of perception, recall and recognition; the course of ideas; the products of the imagination; and the actual con-

[31] W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresber. ü. d. ges. Physiol.*, 1922, 516.

[32] Koffka, *ibid.*

stitution of mental processes. Meaning in perception is explained by means of associative images. When it is a matter of explaining the actual constitution of mental processes, the doctrine of association assumes the form of J. S. Mill's doctrine of mental chemistry, according to which association is not only a cohesive principle but possesses the power to transform or to fuse mental elements. Physiologically, the associational theories have been used to explain psychological facts: for example, in Exner's facilitation theory, residual traces of former excitations in the brain prepare the way for future excitations,[33] thus accounting for memory and perceptual complexes. Von Kries and Becher[34] criticize theories of this type as follows: the theories cannot explain the first association between two or more elements; simple summation does not explain complexity; it is inconceivable on the basis of such theories that a number of excitations could influence one another in assimilative and contrast effects in vision. The configurationists make the most of these arguments and add others: granted, says Koffka, that there are millions of nerve cells and hundreds of millions of connections between them, it is difficult to believe that every perception is conditioned by one set of elements, or that there is time enough for such connections to form in every required situation, *e.g.*, the first time that a baby fixates an object. Köhler points out that there are too many irregularities in the optic sector to result in the perception of homogeneous space if a point-to-point correlation exists between phenomenal and physical spatial structures.[35] And a final objection is that the associational theories suffer from over-simplification: complex processes, meaningful connections, and causally related events do not arise from chance connections in time and space.

If the laws of association reduce to mere existential connection between two or more elements, there is no real reason why these elements should go together or condition one another. In order to account for meaningful connections among the parts of a structure, Koffka has restated the laws of association in the following form: "If A, B, C. . . .once, or several times, have been present in experience as members of a configuration, and if one of them appears bearing its membership character, then the tendency is present for the whole structure to be completed, more or less fully and vividly." The law of similarity, which many associationists reject, would then read: "An idea A can call forth another idea A′ without being bound to it by associa-

[33]Cf W. Wundt, *Physiologische Psychologie*, 3, 1911, 538ff; and S. Exner, Zur Kenntnis von der Wechselwirkung der Erregungen im Zentralnervensystem, *Pflüger's Arch.*, 28, 1882, 487.

[34]*Opp. citt.*

[35]Köhler, *ibid.*

tion if A is sufficiently like A'." Similarity does not fit into associational theories because it represents a real, inner connection between elements which does not reduce to mere contiguity. Configurational theories encounter no difficulties with similarity, even on the side of brain process, because configurational principles apply to structures wherever found. The most general law of the reappearance of structures would then be: "A structure once present in experience creates favorable conditions for its own reappearance or that of a similar structure." In opposition to the associationists, who ask "where" an event occurred, since they are concerned with the elements which are at the bottom of the event, the configurationists ask "what" happens, since it suffices for them to know the specific form of an event in order to predict its behavior.[36]

Theories based upon elements are often relational theories as well. Sometimes it is said that complexes are composed of parts in relation, or that the course of ideas is determined by the "relational consciousness." It has been asserted that the apes react to the relations between the elements, and that these relations remain constant even though the relata change. The configurationists reply that the relational theories do not fit the facts for several reasons. (1) The relations between the containers do not appear to the animal who sees and reacts only to the particular shades or colors involved.[37] (2) The relation-seeing attitude cannot be equated to the "structure-seeing" attitude, because these are two entirely different perceptions and involve wholly different phenomenal patterns; the perception of a structure is prior to and independent of relational judgments. Thus to say that one perceives a square and to say that one perceives that "side *a* equals *b*" are two entirely different judgments involving different objects of perception. (3) As a matter of fact, Köhler denies that, in the example quoted above, the relations are the same; for the gray which stood in the relation of "darker than" in the first situation stands in the relation of "lighter than" its companion in the second situation. But even if it is granted that the relations in transposable configurations are constant, Köhler remarks that this is a rule of configurations but not an explanation. And against Müller, who would explain configurational phenomena by means of relations, collective perceptions and association, Köhler[38] argues that the problem begins where the relational theories stop. Finally, (4) a general criticism of the relational theories is that

[36] K. Koffka, *Die Grundlagen der psychischen Entwicklung*, 1921, 176.

[37] W. Köhler, *Intelligenzprüfungen*.

[38] Komplextheorie und Gestalttheorie, *Psych. Forsch.*, 6, 1925, 382; and *The Problem of Form in Perception*, 7th. Intern. Congress at Oxford, 1923.

they still operate with elements which are somehow external to the relations and given independently of them.[39] The configurationists stress the fact that significant relations are internal, in the philosophical sense, and make a difference to the terms related. A description of parts in relation does not reproduce the configuration with its specific properties as a whole.[40]

(5) *Attention Does Not Explain Configurational Changes*

The concept of attention plays a dominant rôle in many systems of psychology. The configurationists single out those cases in which attention has been used to bolster the 'constancy' hypothesis and association. To get some notion of the many uses to which the word 'attention' has been put, one need only turn the pages of the psychological journals, where it will be found in every imaginable sense: as a function, an act, a condition, a state, an elementary constituent of perception or a separate act in addition to the others, as clearness, vividness and so on *ad libitum*. Koffka links it up with the constancy hypothesis to illustrate one of the worst uses to which it has been put from the configurational point of view. When an object, sensation or association surprises us by its appearance, or by some change which we have not expected, attention serves as an explanation. Attention may change the intensity of the experience,

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[39]The distinction between the form of combination and the formula of combination was clearly drawn by Stout, who points out that every perceived whole involves (1) component parts, and (2) the form of combination of these parts. The problem of the form (-quality) does not concern the relation of combination to the elements combined in the actual constitution of an objective whole, but rather the relation of the apprehension of form to that of matter. "Even though a whole is nothing more than its parts taken collectively, it does not follow that our cognizance of this whole is to be identified with our cognizance of all its parts. It is not a question of mental combination (or chemistry). If apprehension of form is distinct from apprehension of matter, it is not a form of mental combination, but a material constituent of consciousness, comparable to the perception of red or blue." Apprehending the form and formula of combination are two entirely distinct operations. *Cf. Analytic Psychology*, 2, 1913, ch. 3. Husserl as far back as 1891, in *Philosophie der Arithmetik*, pointed out that the perception of the "figural factor" is not to be identified with the perception of the parts in relation.

[40]Becher, in his review of Köhler's *Physische Gestalten*, points out that we must distinguish between systems which are universally causally coherent, in which changing one part influences the rest, and systems which are not. *Cf.* W. Köhler's physikalische Theorie der physiologischen Vorgänge, die der Gestaltwahrnehmung zugrunde liegen, *Z. f. Psych.*, 87, 1921, 1-44. In other words, not all complexes are governed by internal relations. This fact was noted by Lipps, who distinguishes between relations which change the relata, *Verhältnisse*, and those which do not, *Beziehungen*. The former include relations of identity, difference, equality, inequality, similarity and dissimilarity (*cf.* Meinong's ideal relations). Mere spatial juxtaposition is all that results from the external or summative relations. See T. Lipps, *Leitfaden der Psychologie*, 1906, 133.

it may determine the train of associations, or it may leave certain processes wholly out of the field of consciousness. The thought processes and attention may influence each other through association, since attention and association determine the contents of perception and the course of ideas. This factor becomes so variable and capricious that Koffka finds it is impossible to predict what attention will do in a single case; yet it has served as a prop to the atomistic-associationistic theories when others have failed.[41] Let us examine the objections of the configurationists to the use of the word attention as act, determinant and clearness.

One of the typical act-theories which the configurationists reject may be found in Müller's latest exposition of his principles in a volume entitled *Komplextheorie und Gestalttheorie.* Since Köhler has answered Müller at some length we can get a fair notion of the configurationists' position with respect to this very important point by considering briefly the theories of these two psychologists.

Müller begins with elements as his fundamental data, and shows how they are united by collective simultaneous or successive acts of attention. Various factors aid or hinder the perception of wholes. The facts which the configurationists attribute to the properties of configurations as wholes are dealt with by Müller as cases of collective simultaneous attention. Köhler declares that Müller's use of the concept is ambiguous,[42] to say nothing of the contradictions it involves. Again we find a difference of theory leading to different statements of fact: the best configurations do not receive the maximal amount of attention, as Müller's theory would lead us to expect, while the converse is equally true: configurations may receive a maximum of attention and yet remain amorphous, ill-structured aggregates. The acts of attention by which unit elements are combined into wholes are not observable, Köhler points out, nor do they serve any theoretical purpose. In fact the assumption of acts needlessly complicates the explanation of complex perceptions, since, according to Müller's theory, it would be necessary in order to perceive the word "bat" to see the "b" first, then the "a" and finally the "t," after which an act of second order must supervene upon the first three to combine them into a single whole. With words of twelve or more letters we should expect as many more acts. The theory, Köhler concludes, is not justified by observation and is, indeed, quite fantastic.

Another use of the word attention which finds disfavor with Koffka is that of a determinant or conditioning factor, *e.g.*, a determinant of limens. This use implies, first, that there are sensations or other processes which may go unnoticed by attention. From the constancy hypothesis it follows that certain sensations must be present with a given stimulus; and if the observer does not report them, then his attention has been on something else. These unnoticed sensations may, however, play some part in determining what is present in perception.

[41] K. Koffka, *Psych. Bull., loc. cit.*

[42] W. Köhler, *Psych. Forsch.*, 6, 1925, 358 ff.

Thus an observer may report that two grays, *a* and *b*, are equal in brightness, and that *b* and *c* are equal, but that *a* and *c* are not equal. It is assumed that the differences between *a* and *b* and between *b* and *c* must have been present throughout, but that they were too weak to be noticed or attended to. The configurational explanation would dispense with attention and unnoticed sensations and would proceed in this manner: phenomenally, the first and second pair are equal because there is not enough difference to form a "rise" or "fall" from the level on which they appear. But the third pair, consisting of *a* and *c*, forms a new configuration, in which a "step" up or down in brightness can be perceived. The configuration into which the two elements enter determines what is seen. The configurational theories thus need not employ judgments which have been falsely directed, unconscious acts or unnoticed sensations, or elements which have been changed by the dubious activity of attention.[43]

Nor does attention as a purely descriptive term satisfy the configurationists; for this use too supposes that objects may remain constant while their clearness value changes. The changes in objects often ascribed to changes in attributive clearness involve the whole configuration to such an extent that we must regard it as a new object with new properties. Rubin, reviving a distinction made by Helmholtz, Hering, Mach, Wirth and others, shows that when we look at a painting, at objects in a room, or at a scene, certain parts stand out in clear, well-defined pattern while the rest is more or less hazy and undifferentiated and serves as a background for the pattern; one field is figure and the other is ground. The usual explanation of the change from figure to ground (for a field which has been figure may be made into ground) as due to a change in clearness does not do justice to the change; for when figure changes to ground it is transformed into a new phenomenal object possessing new properties.[44] Somewhat analogous results are reported by Fuchs: if a circle is exposed tachistoscopically with a small black dot near its periphery, only a circle on a homogeneous ground will be seen; but if the dot is moved toward the center, where it forms an integral part of the circle structure (Wertheimer's *Schwerpunkt*), it too will be seen. Here the new configuration embodies the dot as an essential part of its structure, and the dot must appear if the configuration is to be seen at all. It is not a matter of clearness; for clearness takes it for granted that the black dot was present in the structure before it was seen, whereas the

[43] K. Koffka, Psychologie der Wahrnehmung, *Die Geisteswissenschaften*, Part 29, 712-716 and 796ff.

[44] E. Rubin, *Visuell wahrgenommene Figuren*, Part 1, 1921. A more exact description will be given in our second article.

structure was not at all present until the dot was seen: in other words, circle and circle-with-black-dot-in-center are two entirely different configurations and must not be taken for the same object of perception. Configurational changes will therefore determine what is to predominate in perception.[45]

Clearness loses its place as a primary concept with the configurationists. It is no longer responsible for *Eindringlichkeit*, *Aufdringlichkeit* and vividness. What will be clear or attended to depends upon configuration or structure, and is explicable from such factors as position within a structure, whether a field is figure or ground, and how the configuration conforms to laws of simplicity, pregnancy or precision (*Prägnanz*), symmetry and the like. Descriptions formulated in these terms show how structures behave, whereas such terms as attention, association, sensation and judgment divorce the content from its form and context and imply that only external modifications have taken place in perception. What is to be clear will therefore follow configurational laws, not the capricious activity of some inner variable.

The question now arises concerning the structure which will be preferred in perception when a number of configurations are possible. We are told that a configuration may be more impressive or may be preferred in perception, imagination or memory for any one of a number of reasons: either it is a "better" configuration, "more natural," or, having appeared a number of times before, it has left a "configurational disposition" making its reappearance on subsequent occasions easier. Attitude, set, habit (*gesamter psychischer Habitus*) all tend to facilitate or inhibit certain configurations.[46]

(6) *Relation of Configurationism to Other Schools*

Questions and problems which have been the bugbear of other schools of psychology receive short shrift from the configurationists, who act upon the maxim given by Goethe to a friend: "The greatest art in theoretical and practical life consists in changing the *problem* into a *postulate*." The problem of complexes and relations is turned by the configurationists into a postulate, and the riddle is solved. Asserting that the whole determines the nature of its parts, the configurationists are certain that if we describe and explain the total configuration its parts will take care of themselves. The fundamental data for them are therefore objects, patterns, forms, as these are given in immediate experience. In place of the sensation or the mental element as the primary unit behind which we shall not enquire,

[45] W. Fuchs, *op. cit.*

[46] Wertheimer has been foremost in coining expressions for the peculiarities of configurations. *Cf.* his articles in *Psych. Forsch.*, 1922 and 1923.

the configurationists substitute the total structure or meaning of any given mental process or response. If any analysis is to be made, it must be in terms of conditions which affect the total situation without destroying it. All this means that we cannot separate stimulus from perception, quality from form, or a part-response from the total situation in which it is found.

The fundamental configurational concepts can be traced to several different sources. Ehrenfels laid down the two fundamental criteria of configurational structures in a paper published in 1890 on the form-quality (*Gestaltqualität*): a configuration possesses properties over and above its parts and not derivable from them; the configuration is transposable since it does not depend upon any given set of elements. Any change occurring in a definite direction, according to Ehrenfels, is a form-quality, *e.g.*, reddening, blushing, cooling, growing blue, or a rise in scale. Relations, too, are form-qualities, but in a slightly different sense, for they require the activity of a subject. But Ehrenfels' actual definition of the form-quality does not find favor with Wertheimer, because it savors too much of the atomistic presuppositions of the associationistic psychology: the form-quality is a definite ideational content appearing in consciousness and composed of separable elements. Various difficulties with the concept arose, especially with regard to the relations of the contents, and Meinong elaborated the theory in an endeavor to bring relations and contents together. To explain the emergence of the form-quality within a given complex, Meinong introduced a number of terms, chief of which was that of a special activity acting upon sensory elements to produce the unitary, structured whole.[47] Meinong's theories have figured prominently in the writings of Witasek, Höfler, Benussi and other members of the Graz school, who may perhaps be designated as the "left wing" of the configurational movement.

The disagreements between the older and newer theories have been amply set forth by Koffka, who assails Benussi for introducing the production-process (*Produktionsvorgang*) to explain how sensory elements are united into complex wholes. The followers of Meinong retain the sensation as the constant unit in perception, in immediate one-to-one correlation with the stimuli, while the form, pattern and structure arise from a psychical activity central in origin. Koffka and Köhler reject

[47]The controversy over the status of the form-quality and of complexes and relations in general involved Meinong, Witasek, Höfler, Benussi, Cornelius, Schumann, Gelb and a number of others. The problems reappear in the present discussions of the configuration, but in the modified form that we should expect after the twenty years of psychological research which have intervened since the controversy was at its height. For a review of the discussion and bibliography, see Bentley, *op. cit.*, Note 4.

this division of conscious contents into two classes, and insist that configurations have objective validity, inherent in physical systems. The new configurationists, far from making the configuration subjective, have constructed an elaborate physical and physiological theory (which has undergone several modifications) to prove that phenomenal configurations may copy, or may spring directly from, physical configurations, without the intervention of any concepts peculiar to psychology or vitalistic in character.[48]

Another point of difference between the new configurationists and the left wing concerns the origin of configurations. The configurationists are by no means agreed among themselves on the finer points of their theory here, but there is enough agreement to allow us to speak in general terms for all. Meinong, Witasek and Benussi imply that the elements are prior to the form or pattern in which they appear. The opposite view is held by the new configurationists. Koffka believes that the configuration precedes the parts, in the history of both the individual and the race. Wholes are no less real than their parts. Nor can parts be isolated from wholes at random, says Köhler, and still remain true parts. "Von diesen [Gestalten] aber können nicht beliebig herauszuschneidende geometrische Stücke als Gestaltteile gelten."[49] The natural parts of a configuration seem to be those portions of the configuration which can be recognized as belonging to a given structure even though they are isolated from the whole: thus the number 8 has only two natural parts, the upper and lower half, because these come the nearest to suggesting the original.

So far the discussion has hinged mainly upon perception; and this restriction is not out of keeping with the stress laid upon perception by the configurationists. Their theories originate in perception, and find most fruitful application in that field. But the configurationists feel confident that their concepts are applicable to every field of psychological investigation; and this belief brings them into connection with several other important schools of modern psychology. Although the configurationists regard the fundamental assumptions of the imageless-thought movement as atomistic, they agree with Watt, Bühler and Messer that meanings, goals, and any factors which give direction to mental life or behavior must become part of experimental psychology. The determinants of mental life are not, however, new elements (Bühler's *Bestandstücke*), impalpable contents or unobservable tendencies; rather, to quote Wertheimer, "we conceive of an inner necessity, of mean-

[48]The publications in which Koffka, Benussi and Köhler express their views on this important topic have been cited in Notes 7, 16, and 21.

[49]W. Köhler, *op. cit.*

ingful predictions and completions wherein events do not occur through blind, outer factors, but through inner demands. Thus (*contra* Hume) we can predict the locus of a curve through inner necessity, an actual inner motive which we do not regard as a purely psychologically fortuitous set of events."[50]

Another aspect of configurational theory borders closely upon act-psychology. We cannot separate perception from memory, thought and action.[51] Perception leads over into action and is a necessary condition of action. Phenomenal patterns determine reaction patterns. Afferent, central and efferent portions of the nervous system must be regarded as interdependent parts of a single, causally-coherent system, in which every part influences and is influenced by every other. That the phenomenal pattern may determine reaction patterns was shown by Köhler in this way: a rope wound around a beam presented insuperable difficulties to the apes; what appears to us as a clear, articulate structure evidently seems to these animals like a tangled, intricate structure. The apes react to the rope around the beam as we should react to a knotted skein of thread,—tugging, pulling and scratching, but never unwinding. The animals' reactions are accordingly as meaningless and poor as ours are in the face of a situation which is beyond our powers of discrimination and comprehension.

The configurationists do not regard the formal properties of experience as the correlates of special acts. Contents are in constant flow and point beyond themselves to other contents and to functional situations. The organism is not a passive receptacle for adventitious stimuli; rather, it selects, reacts to, and molds the situations which confront it. The changes in phenomenal and behavior patterns represent dynamic processes in time, which cannot be resolved into static units of an elementary character.

The configurational method and descriptions are largely phenomenological. Some of the objections to the traditional concepts raised by the configurationists come directly from Husserl, or can be traced to a thoroughgoing application of his principles to psychological problems. Their emphasis upon descriptions in terms of observable data, their inclusion of meanings in psychology, and their insistence upon preserving the essence of any given experience, all alike owe their origin to phenomenology.

Phenomenologically, experience presents us with self-contained wholes whose meanings are resident within the immediate data themselves and cannot be found in accruing or correlated

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[50] M. Wertheimer, *Psych. Forsch.*, 1, 1921, 57.

[51] *Cf.* F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psych. Forsch.*, 1, 1922, 333-373.

processes. The phenomenological method is positivistic and empirical. Deductive theorizing, logical constructs, abstraction and inference must be brought to the bar of immediate experience; thus association, the concept of the mental element, the mechanisms and processes used as explanations of the facts of perception, memory or thought are submitted to factual tests. We can therefore understand the configurationists when they assert that form, pattern, figure-and-ground, good, bad, hard, soft, precise, and articulate describe the facts of configurational structures and must be included in psychology. The perception of objects and complexes yields very readily to this kind of description. Configurational descriptions usually assume the form of statements of differences in *kind*, rather than in *degree*, since quantitative units imply that meanings and wholes can be built out of fundamentally similar units by purely additive operations. The configurationists hope to unify the facts of psychology by reducing diverse configurational phenomena to similar sets of conditions to which they owe their origin and development.

Emphasis upon total situations implies relativism. The configurationists are the *echte* psychological relativists.[52] Their relativism harmonizes with their rejection of constant elements and of the importation of subsidiary factors to help explain what is actually observed. It means that we cannot tell from the parts alone what the whole will be; every situation is a function of a total set of conditions in which it is enmeshed. Once these have been discovered, the whole appears to be internally governed or autonomous. Indeed, within a given whole many opportunities may be present for modifying the structure: position, quality, and a number of other variables may change a configuration or its parts.[53]

At this point the question arises: "What innovations do the configurationists actually propose, and what are the systematic implications of the new psychology?" Perhaps the most radical proposal of the configurationists is that objects, meanings, affective, evaluative and aesthetic categories must be included within experimental psychology. If we begin with the assumption that objects and forms are immediately given in experience, it follows that affective and evaluative experiences preceded analytical and discriminatory responses and hence must be re-

[52]G. Humphrey, The Theory of Einstein and the Gestaltpsychologie; A. Parallel, this JOURNAL, 1924, 353-359. I do not subscribe to all that is contained in this paper. In fact, according to the exposition, it will be clear that I am concerned only with the psychological aspects of configurational relativity.

[53]W. Fuchs, Experimentelle Untersuchungen über die Änderung von Farben unter dem Einfluss von Gestalten (Angleichungserscheinungen), *Z. f. Psych.*, 92, 1923, 249-326.

garded as no less fundamental and primary than red, sour and extensity. Certain objects excite fear in and for themselves. To analyze a fear-response into anything else would be to destroy what alone has meaning in the fear-situation, both perceptively and behavioristically. So also the meanings sharp, round, square cannot be analyzed into elements which are none of these. The psychology of perception and behavior must be rewritten on the new basis.

But the phenomenological point of view does not drive the configurationists into a purely descriptive psychology; the concept of configuration is too all-inclusive to allow of that. Perceptual and thought patterns are involved in a net-work of physical and physiological conditions which must always be taken into account. The configurationists hope to bridge the gap between psychology, biology, and physics. This extension of the concept of configuration results in a variety of meanings of the word. Hence we must try to discover a little more exactly just what the *Gestalt* stands for, and how we are to identify configurational phenomena in experience. This must be done before we can answer the question raised above.

(7) *Various Applications of the Concept of Gestalt*

The word *Gestalt* has been used in a variety of ways by members of the new movement. Usually there is agreement in intension but the concept is variously applied in extension. On the phenomenological side we find several different meanings along the following order. The configuration may refer to any given totality described in terms of its meaning. Or the configuration may mean the form of a group, *e.g.*, square, triangle or circle. The essential designation may be the manner of combination of a group of units into a whole: four dots may be perceived as a square, as lines in various positions, or as a tilted cross (Benussi's *Gestaltmehrdeutigkeit*). Often the configuration may include factors at present unknown. Physiologically a configuration consists of the afferent, central and efferent portions of the nervous system functioning as a unit: there are no isolated paths, no single point-excitations connected by strands; the form of the excitation and its spread are here of importance. From the point of view of behavior a configurational response may be defined as a reaction pattern in which each act attains meaning and is what it is by virtue of its part in the total situation confronting the organism. The actual definition given by Köhler states the configurational point of view very clearly in a few words: "An objective consideration of the behavior of the apes must show more and more clearly that if the single acts are considered in isolation they appear to possess no meaning so far as the problem to be solved is concerned; but if they are taken in connection with the total process they are related parts of a (specific) whole and are therefore intelligent."[54]

The general definition of configurations given above is sufficiently wide to embrace several universes of discourse. There are physical, physiological, biological and psychological structures possessing properties not compoundable from their parts and hence to be regarded as configurations (Köhler). The configurational principles are not peculiar to psychology; Köhler feels that he asserts nothing in principle which would

[54] W. Köhler, *Intelligenzprüfungen*, 72.

not receive the assent of physicists and biologists.[55] It should be possible, and indeed it is necessary, he thinks, that psychology employ concepts which shall embrace physical, biological and psychological facts within a single system. With concepts common to all, the chasm between psychology and the other sciences can be bridged. First of all psychology and biology find common ground in the fact that "the striking harmony of the structure, function and reactions of the living organism illustrates the unity and completeness which differentiate organic from inorganic life." This granted, another step may be taken which will carry us over to physics. If there are physical configurations possessing properties not compoundable from their parts, it may be possible to regard central physiological processes in the nervous system as special cases of physico-chemical configurations. Physics and psychology will then employ common assumptions; and the transition can be easily made, *mutatis mutandis*, from body to mind, from stimulus to perception, and from environmental situations to phenomenal processes and reaction patterns.[56]

This programme is indeed far-reaching. The concept of the configuration becomes almost universal in application. It is to be expected that what it gains in universality it loses in specificity and uniqueness; hence the variety of meanings of the term *Gestalt*. We cannot be sure in any given case whether the facts referred to are psychological, biological, physical or logical. There are therefore at least four different domains in which configurational phenomena may be found, each one with its own specific characteristics. Whether the properties of a physical or logical structure can be carried over bodily to psychological configurations cannot be determined in advance; and unless every case is examined on its own merits, it is impossible to predict where the different disciplines will or will not meet on common ground even with reference to configurational structures. Let us examine the concept of the configuration in physics, physiology, biology and logic a little more closely.

The physicist, thinks Köhler, does not investigate summative groupings of matter or electrical charges; they offer him no problem. He is interested rather in studying the properties of material systems in which each part is influenced by every other. Although summative groupings of matter may be found in nature, a true physical system is more than a sum of parts. The classical example of a non-summative grouping is found in a static charge distribution. If an electric current is conducted to an isolated wire, its total charges quickly come to a state of equilibrium and the whole surface of the conductor has the same potential. This static grouping remains the same so long as no other disturbing factors are present. Such a state of equilibrium Köhler calls the charge distribution. This distribution is not a mere sum of part charges, since electricity cannot be taken away from any part of the system without influencing every other; when this is done, a new state of equilibrium is effected, which represents the new charge distribution. The first criterion of configurations, enunciated first by Ehrenfels, therefore applies to physical systems in that they too possess properties which are characteristic of the whole structure: the physical system possesses a certain potential, a specific density at every point, and a pattern representing the distribution of energy. The second criterion, that of the transposability of structures, applies also to these systems; for if one doubles the potential, the charge density doubles at every point. Stated generally, proportional changes in the size of the conductor and of all other factors in the situation do not change the distribution of charge. The distribution depends only on the form of the conductor. And if we substitute for the observer a single conductor, then,

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[55] W. Köhler, *Physische Gestalten*.

[56] W. Köhler, ibid., Introduction.

just as the notes of a melody must be perceived by some one person in order to be heard as a melody, so must the charge be carried by a single conductor.[57]

On the basis of his physical principles Köhler erects a physiological theory according to which psychophysical configurations represent special cases of more general physico-chemical processes.[58] The events in the nervous system appear in many respects like reactions in diluted solutions. Processes in the nervous system are largely loosened matter, electrolytically dissociated. Ionic reactions play an essential part in conduction. If the nervous excitations are constant, the chemical processes in the somatic fields are stationary, and the ionic concentrations have a constant value. Excitations in the somatic field with constant outer conditions are quasi-stationary chemical reactions, in dilute solution, in which ions participate. Therefore at every moment the excitation states are determined by the concentration of the reacting ions. If two different parts of the periphery are stimulated in a constant manner, these will represent two part-domains differently stimulated but which bound each other in some kind of a continuous curve. In the somatic field there will be two corresponding part-domains bounding each other, but with different ionic concentrations. Owing to diffusion and to the different velocities of the positive and negative ions there will be a drop in potential or electromotive force in each field. This drop is a function of the kind of ions concerned in the process and of the amount of their respective concentrations. Important consequences for the various fields of perception, only one or two of which can be mentioned here, follow from these facts.

According to this theory, the nervous system is highly permeable and contains no isolated segments functioning independently of the rest. In fact, Köhler maintains that we must regard the retina as a part of the central nervous system functionally as well as anatomically, so that the whole afferent and efferent optic sector forms a single system. Contours in visual perception would then represent bounding portions of two differently colored fields in which there is a difference of potential. The amount and direction of the change in potential are determined by the color excitations: as the colors approach each other in quality the potential difference disappears and at the same time the contour vanishes. The contour is (as Rubin pointed out) not a mere geometrical boundary, but is determined by both the fields out of which it arises.

The way in which Köhler applies physical configurational principles to psychological configurations may be shown by reference to his explanation of figure-and-ground. If we perceive a small white circle on a gray background, we may assume that two quasi-stationary reactions are set up in the retina, the one corresponding to the figure, the other to the ground. There will be different ionic concentrations in each area. Between the white and the gray there will arise a difference in potential which is responsible for an electromotive force between them. Furthermore, there will be differences in potential between the stimulated portions of the retina and the optic nerves, since it requires time for the chemical reactions to occur in long-section as well as in cross-section. Because of the differences in potential electric currents will arise, travelling from the retina through the optic nerves and from one portion of the retina to the other. The current will be stronger and more dense in the smaller, circular area than in the broad gray background, for the simple reason that the energy of the system will be more strongly concentrated in the smaller area than in the larger. Since the energy per unit area is greater in the figure than in the

[57] *Ibid.*

[58] The exposition of this and the following three paragraphs follows the argument of Köhler's *Physische Gestalten*.

ground, the figure will possess greater impressiveness, and will therefore predominate in perception. This reasoning applies just as well when the figure is gray and the background is white, so that the vividness of the figure is not a matter of color or brightness effect so long as the colors are different. Association, attention, and imaginal additions are unnecessary to explain the predominance of figure over ground, since the laws of configurational structures explain it on the basis of what goes on within the structure itself.

Köhler further believes that the new point of view can be applied to the explanation of the thought processes. The appearance of complex unities possessing specific structures in the physical world which are copied in perception leads us to think, he says, that even the higher thought processes are accompanied by configurational processes within the nervous system. It had always been an objection against the assumption of a physical correlate of the higher thought processes that unities possessing specific structures could not be correlated with any corresponding brain processes. Configurational principles enable us to achieve this correlation.

Up to this point the road from physics to psychology has seemed fairly clear. But it is not always possible to translate directly from physics into psychology. The configurationists often find themselves compelled to resort to biological and logical principles in order to supplement their descriptions and explanations of psychological configurations. Biological conditions may determine phenomenal and behavior patterns. Thus we are told that the animal and the child do not react to an environment composed of elements nor by means of unit reflexes. An emergence from a general level greets the newly-born infant, rather than patches of colors with a certain hue, saturation and tint. Primitive reactions are to functional situations by means of total responses. Chain-reflexes knit together by trial and error, stamped in no one knows how, cannot explain the activities of the child or of the hungry ape. Discrimination is a product of later development, and consists in learning to react to finer and finer structures. Biologically such terms as "good," "bad," "dynamic," and "natural" represent configurational properties of the environment and the kind of response which the organism makes to it.[59]

Still another field furnishes the configurationists with descriptive categories and criteria of configurational processes,—that of logic. Wertheimer draws very heavily upon this source. This author stresses the fact that configurations are governed by "inner" as opposed to "outer" necessity, the inner and outer referring now to internal *versus* external relations, and now again to the forces at work within a configuration as opposed to the outer conditions. Logical meanings and implications may determine what is seen in perception or what the train of associations will be in thinking. Whereas Köhler means by "simplicity," "economy," and "precision" physical or biological facts, Wertheimer usually employs the terms as logical concepts applied to psychological facts. When Wertheimer writes that the "whole determines the nature of its parts," we cannot be sure whether the whole referred to is the total psychological structure, the whole logical meaning, or some other product of reflection. A typical sentence from Wertheimer shows his logical emphasis very clearly: "dabei muss klar sein dass z. B. Symmetrie durchaus nicht einfach eine 'Gleichheit' von Teilen ist, sondern logisch richtig nur vom Ganzen her, als Ganzeigenschaft gefasst werden kann."[60]

The question arises how far logical, phenomenal and physical configurations obey the same laws and harmonize with each other. We find that in some cases phenomenal fields may copy physical fields, while in others they do not. Physical and psychological simplicity may or may not coin-

[59] K. Koffka, *The Growth of the Mind.*
[60] M. Wertheimer, *Psych. Forsch.*, 4, 1923, 325.

cide; *e.g.*, a straight line is geometrically simple and perhaps, in some sense, physically simple also; yet it cannot be considered as a configuration for a number of reasons, and mainly because it lacks the property of completeness (*Geschlossenheit*). So too "good," "bad," "precise" and "natural" mean differently according as they bear a physical, biological, logical or psychological stamp. The lines are therefore not sharply drawn, and so it is impossible to fix precise meanings for the concept of *Gestalt* when we examine it closely in its various uses in the configurational literature.

(8) *Some Systematic Implications of Configurationism*

What are the general implications of configurationism viewed in their systematic setting? I can point these out very briefly in recapitulating the sum and substance of this paper.

The most striking development in the configurational psychology seems to consist in extending the boundaries of the psychological to include the facts of biology, physics, and logic within a single system deducible from a common set of assumptions. I have already pointed out how configurational concepts are used in this task, and I leave it for later discussion to decide on the merits of the question. The emphasis put upon physical and physiological hypotheses indicates a tendency among members of the new school to seek objective explanations of phenomenal facts, or at least to employ principles common to what we may call, for convenience, the "objective" sciences. Mind thus becomes a phenomenon within a larger system of phenomena and amenable to precisely the same laws, with no peculiar status or properties. Strangely enough, this point of view does not lead to behaviorism but rather toward greater emphasis upon central neural processes as opposed to peripheral correlates of phenomenal patterns. The configurationists hope to make the transition from brain process to conscious process without any break in theory.

In order to approximate the facts of everyday life more closely and to avoid the artifacts of laboratory procedure, the configurationists substitute phenomenal patterns in place of elements as the typical data of observation. These patterns, they assert, are no less immediate than the sensory components which Benussi and others find to be given first in perception. The aim of experimentation, according to this point of view, should be to describe and explain the origin, growth, and decay of configurational processes both in the nervous system and on the "psychophysical level" where phenomenal patterns appear. No assumptions or theories regarding the ultimate constituents of consciousness find a place in configurational psychology. While it is admitted that the only basis for a choice between a description in terms of elements and one in terms of configurations is pragmatic, it is the belief of Koffka, Köhler and Wertheimer and their followers that the "best" scientific descriptions will be formulated in terms of the configurational properties of the observed facts.

The new point of view implies new functional descriptions as well as a new treatment of perception. The typical form of psychophysical connection, stimulus-experience, is no longer the sensation but rather the configuration. The configuration must be regarded as the correlate of the stimulus just as was formerly the sensation. While configurations requiring time for their completion offer the configurationists many difficulties, they feel that the concept of the *Gestalt* is designed primarily to embrace dynamic processes extended in time as well as in space. Motor reaction-patterns are determined in their temporal order by the end or goal which may be an integral part of a configuration even though it is not present in time and space. Part-activities can thus fit into meaningful ordered wholes for which the ground of connection is the concept of the configuration.

In place of the older descriptions in terms of elementary processes and their attributes, the configurationists speak in terms of configurational changes somewhat in the following fashion: configurations become more articulate or more finely structured, simpler in form, more precise or less definite, poorer in form or chaotic. What is to be singled out in perception or memory will depend upon the position of the part within the whole, whether or not it is a natural part and will lend itself to membership in a new configuration, and upon a number of other factors like coherence, persistence and impressiveness. In general, the progress of configurational change is from chaotic groups of elements, at the one extreme (really a minimum of "structuration"), to more and more complex structures. The development of perception, memory, imagination, thought and action consists in the development of configurational patterns in ever-increasing complexity and variety within a given form.[61]

Which comes first, the sensation or element without form or order within a group or the configuration? This question is answered in many ways, according to the point of view of those who have tried to answer it. We should expect to find differences between the configurationists on the one hand and members of other schools on the other. But there are "half configurationists," as well as the left wing represented by the Graz school; and the configurationists themselves have formulated various theories of the origin of configurations. We survey the various possibilities, in order to resume *in nuce* what the leading writers have said on this point.

It will be remembered that the followers of Meinong, particularly Witasek and Benussi, maintain that stimuli give rise to sensations which are united into patterns and configurations by special productive acts. Several configurations may arise from a given set of elements, with no possibility of predicting which will be favored. Koffka objects to this position on the ground that a strict division of conscious contents (sensation

[61]K. Koffka, Introspection and the Method of Psychology, *Brit. J. Psych.*, 1924, 15, pt. 2, General Section.

and production-process) is impossible; furthermore, sensations are no more stable than configurations, and to give the former a real status as against the ideal status of the configuration implies the 'constancy' hypothesis in its worst form. Koffka drops the sensation as the immediate correlate of the stimulus and puts in its place the configuration, which thus appears first in the history of the race and of the individual.[62] Köhler derives the properties of phenomenal configurations immediately from physical structures, and endeavors to account for discrepancies between the two by reference to irregularities in the sense organs or nervous system.[63] Wertheimer's paper of 1912 seems to imply that single excitations may give rise to total physiological processes, which in turn are accompanied by phenomenal configurations; the change from singleness to group-unity seems to take place in the nervous system. Bühler[64] assumes that simple configurations cannot be derived from sensational processes, although complex configurations may be built out of simpler and resolved into simpler again. Linke[65] extends the notion of configuration to include all the "formal" aspects of experience; the "material" of experience is sensational, but always comes to us in some form or pattern. Still another view has been stressed by Poppelreuter, who worked with pathological observers.[66] His investigations demonstrate, he believes, that structures, forms, and patterns do not appear in perception until very late. Sensory contents may be present in an amorphous state with no hint of structure or form. The perception of configurations requires higher physiological mechanisms which may be destroyed while the sensational mechanisms remain intact. On the extreme sensational side stands Müller[67] who asserts that elements must be united into groups by acts of attention.

We have thus run the entire gamut of possibilities regarding the origin and nature of configurations and their parts.

(9) *Conclusion*

From this general statement of principles it is clear that configurationism draws its inspiration from many different sources. It reaffirms, in modified form, the facts enunciated by Ehrenfels, demanding that the morphological aspects of experience receive consideration at the hands of experimental theory. Configurational structures are resident within perception, thought and action and give them meaning, order and value. With the members of the imageless-thought school, the configurationists assert that the thought processes must be regarded in their temporal, meaningful connections if their significance is to be explained. The active dynamic aspects of mind and body find an explanation not in the unobservable acts of the followers of Brentano, but rather in a union of act and content, and of mind and body, as they function in a material, biological and social environment. The fundamental explanatory and descriptive terms of our science must be observable and capable of factual verification. Experimental psychology must turn from the artifacts of laboratory procedure and systematic bias to the everyday experiences of actual life.

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[62]K. Koffka, *op. cit.*, *Z. f. Psych.*, 1915.
[63]W. Köhler, *Physische Gestalten.*
[64]K. Bühler, *Die Gestaltwahrnehmungen*, 1913.
[65]P. Linke, *Grundfragen der Wahrnehmungslehre*, 1919.
[66]W. Poppelreuter, *ibid.*
[67]G. E. Müller, *op. cit.*

THE PHYSICAL BASIS OF THE CONDITIONED RESPONSE

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I. Introduction

The prominence which the conditioned-reaction principle has attained in modern psychology seems to justify a closer study of the physical factors involved, and the present paper attempts to supply this deficiency. We do not assume that the conditioned response is the *only* form of association, but we attempt to show that it receives considerable support from nerve-physiology, physics and chemistry. The distinctions ordinarily made between physics, biology and psychology are in the last analysis artificial and principally for academic convenience,--and a certain amount of harmony on the physical side will clarify several of the problems involved in the psychology of learning.

The particular experiment which will be used for illustrative purposes in this paper is one which was carried out by the writer on the pupillary reflex.[1] In this experiment, as in another study, on the conditioned eyelid reaction,[2] the "voluntary factor" criticism of the conditioned reaction was eliminated;[3] in the former case because the pupillary reflex could not be controlled "voluntarily," and in the latter case because the time of the conditioned eyelid reaction was made faster than the so-called voluntary factors can make it. In the pupillary experiment, the *S*s were not aware of the act which was learned; and in the eyelid experiment they were not conscious of winking until after they had winked.

In the pupillary experiment just referred to, an increase in the intensity of light falling on the *S*'s eye caused a contraction of the *sphincter iridis* muscle. In this way contractions of the pupil were evoked 400 times at intervals of about 15 sec.; and while the *S*'s pupil was contracting in this training series a bell-stimulus was also present. After this rather long training

[1]H. Cason, The Conditioned Pupillary Reaction, *J. Exper. Psychol.*, 5, 1922, 108-146.

[2]H. Cason, The Conditioned Eyelid Reaction, *ibid.*, 5, 1922, 153-196; A Note on the Conditioned Eyelid Reaction, *ibid.*, 6, 1923, 82-83.

[3]According to this criticism, the *S* is always aware of the learned act, and has a "desire" or "wish" or "impulse" to acquire the new reaction-tendency.

period, the bell-stimulus alone would contract the pupil. A connection was obviously formed between the end-brushes of axones coming from the cochlear division of the eighth nerve and the dendrites of cell-bodies whose processes innervate the *sphincter iridis* muscle directly or indirectly. This association may have been formed in the oculomotor nuclei, or more probably in the temporal lobe of the cerebrum. Before the training period for each *S* was begun the bell caused a slight dilatation of the pupil in the case of every *S* tested. The nervous connection between the bell and a contraction of the pupil was therefore not in functional activity at the beginning of the training period, and the experiment illustrates clearly the way in which an acquired reaction can be actively built up on the nervous structure already present. It is very arbitrary to suppose that

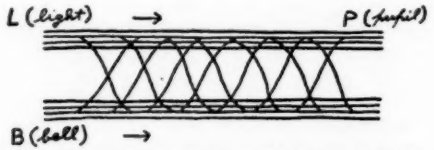

FIG. 1. Showing the nervous path from the retina to the *sphincter iridis* muscle (L to P), together with certain nerve fibres which were innervated by the bell-stimulus (B). The fibres passing from the retina to the *sphincter iridis* muscle and those innervated by the bell intermingle at several points.

a conditioned reaction can be established only when there is a "tendency" already present in the nervous system. The nerve fibres are there, to be sure, but they are not in functional activity. We could with just as good reason suppose that there is a "tendency" in the nervous system for any stimulus to evoke any response.

The path between the retina and the *sphincter iridis* muscle was innervated every time the light-intensity was increased, and this path is represented diagrammatically in Fig. 1 (L to P). The bell-stimulus (B) was also present every time this path was functioning. While we have little detailed knowledge of the reactions ordinarily called out in the human organism by the sound of an electric bell, the bell-stimulus certainly innervated several neurones whose end-brushes are in close proximity to the path from L to P. Since the bell-stimulus alone would later cause a pupillary contraction, some such intermingling as is represented diagrammatically in the figure must be accepted.

Fig. 2 represents a unit of Fig. 1. The nervous path between the light and the pupil was in very good working order during the experiment, and one of the synapses in this path is represented in the figure. An end-brush of one of the neurones

innervated by the bell, which at the beginning of the experiment was not functionally connected with P, is also represented diagrammatically in the Fig. by B. At the beginning of the experiment, the impulses coming along B may or may not have passed over to another neurone S. As stated above, the bell-stimulus caused a dilatation of the pupil at the beginning of the experiment in the case of every *S* tested. Some of this effect may have persisted throughout the experiment, but the external results showed that the tendency toward contraction was relatively more prominent than the tendency toward dilatation at the end of the experiment. Some impulses may have continued to pass from B (in Fig. 2) to cell-bodies whose processes caused

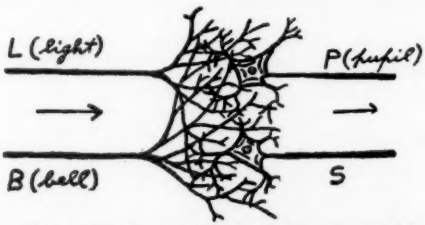

FIG. 2. Showing a unit of the situation represented in Fig. 1. L is an end-brush of one of the neurones innervated by the light-stimulus, and B an end-brush of one of the neurones innervated by the bell-stimulus. P represents a cell-body in the path from an increased light-intensity to a contraction of the pupil. The synapse between L and P remained in fairly good working order throughout the conditioned reaction experiment described in the text. Neurone S indicates the course which an impulse coming along B may or may not have taken at the beginning of the experiment. The two end-brushes, L and B, end in close proximity to nerve-cell P.

various reactions in the body, such as vasomotor changes, alteration in heart-beat, breathing, etc., the exact nature of which is not known. S in Fig. 2 arbitrarily represents all of these changes in the body ("secondary reactions"), in addition to a dilatation of the pupil caused originally by the bell-stimulus. The two end-brushes in the figure (L and B) are shown ending in close proximity to nerve cell P. In our conditioned-pupillary-reaction experiment, L and P were *active* simultaneously 400 times. Every time they were active, however, end-brush B was *also active*. At the *end* of the experiment, but not at the beginning, the stimulus from L could be omitted, and an impulse would pass from B to P. Our task in the present paper is to give an explanation of the formation of this association between B and P. Before proceeding with this explanation, however, we shall have to consider a modern and also a very

plausible theory of conduction in nerves, that is, the theory which has been developed by Nernst, Lillie, Lucas, and Troland.[4]

II. The Nature of Nervous Conduction

The following account of the theory of nervous conduction is largely based on the account given by Troland,[5] to which the reader should turn if a statement of the experimentally demonstrated facts which support the theory is desired. The main features of this theory which concern us particularly are as follows.

A neurone is made up of an aqueous solution of various substances ordinarily held within the cell by a more solid differentiation in structure,—the cell membrane. Certain of these substances become ionized in solution and an equal number of positive and negative ions are formed. The ions of carbonic acid are formed by the respiratory function of the cell.[6] Since the cell has a temperature considerably above absolute zero, all of the molecules and ions of the cell are in rapid motion. The motion of the dissolved particles in the solvent, water, is similar to that of the molecules of a gas in free space; and these particles exert a pressure on the cell membrane. The positive hydrogen ion is the smallest known particle, and it will get through this semi-permeable membrane. The carbonate ions, which are much larger than the hydrogen ions, are held back by the membrane. In time a state of equilibrium is reached between the positive hydrogen ions on the outside of the membrane and the negative carbonate ions on the inside. The mutual attraction between these two layers of ions causes the hydrogen ions on the outside to remain in the immediate vicinity and limits the number of hydrogen ions which can pass through the membrane. The magnitude of this "electrical double layer," or polarization, depends upon the permeability of the membrane and the concentration of the ionized substance.

In order to stimulate the nerve cell, the electrical current must *depolarize* the membrane to a certain critical degree. This assumption seems to harmonize with various facts concerning nerve-functions, including Pflüger's law of electrical stimulation.[7] The cathode sprays the nerve with electrons, and these negative charges neutralize the positive hydrogen ions on the outside of the membrane and depolarize it. The anode takes electrons away from the outside of the cell, thereby increasing the layer of hydrogen ions at this point. The membrane compensates for this increased polarization.

To quote from Troland: "It is evident that the Nernst theory of stimulation in the modified form outlined by Lillie demands a 'negative variation' of the 'current of rest,' as empirically found, since depolarization would necessarily manifest itself in this way. It does not follow, however, from any statements made heretofore that this negative variation will be

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[4]W. Nernst, Zur Theorie des elektrischen Reizes, *Arch. f. d. ges. Physiol.*, 122, 1908, 275-315; R. S. Lillie, The Relation of Stimulation and Conduction in Irritable Tissues to Changes in the Permeability of the Limiting Membranes, *Amer. J. of Physiol.*, 28, 1911, 197-222; K. Lucas, *The Conduction of the Nervous Impulse*, 1917; L. T. Troland, The Physical Basis of Nerve Functions, *Psychol. Rev.*, 27, 1920, 323-350; and other papers.

[5]*Op. cit.*

[6]A solution of carbon dioxide in water (H_2CO_3) has the properties of a weak acid. Carbonic acid conducts electricity, but not well. The bicarbonate ion (HCO_3^-) itself ionizes to form a hydrogen ion (H^+) and a carbonate ion (CO_3^{--}). The ion, HCO_3^-, is an extremely weak acid.

[7]For a brief account, see W. H. Howell, *A Text-Book of Physiology*, 1918, 88-90.

propagated along the cell or fibre from the point of stimulation. Lillie has pointed out that to explain propagation of the variation we must suppose not only that the polarization of the membrane depends upon its differential permeability but that its relative impermeability depends upon its degree of polarization. Polarization and permeability, in other words, are the two essential factors in the nerve process, and they are related to one another in the propagation of the nerve impulse in a manner analogous to the relation of pressure and displacement in the propagation of sound or the relation of the electric and magnetic vectors in the propagation of light. These relations are all such that a change in one of the quantities always involves a change in the other, the locus of the secondary change not coinciding with or falling wholly inside of that of the primary change. This relation necessitates propagation."[8] The permeability is altered when an electric current depolarizes the membrane, because of the removal of the electrical lines of force which extended through the polarized membrane. These electrical lines of force which connect positive charges on the outside of the membrane and negative charges on the inside necessarily affect the structure of the membrane, and this distortion renders the membrane less permeable. Stimulation of a cell therefore increases the permeability of its membrane. "Tashiro finds that a small amount of carbon dioxide is given off by the nerve fibre even in the resting state, but that this amount is markedly increased during stimulation. This indicates that the normal permeability of the membrane is such as to permit a slight diffusion of carbonate ions and that the depolarization accompanying excitation is the result of an increased permeability to these ions rather than a decreased permeability to the positive hydrogen ions."[9]

The system, however, has a point of unstable equilibrium, as the following statements by Troland indicate: "An increase in permeability always produces a decrease in polarization. An initial decrease of polarization, however, first results in a decrease in permeability which tends to compensate for the initial change. But if the depolarization reaches a critical or threshold amount the 'sign' of the change is reversed, so that an increase in permeability results. As soon as this increase begins, the initial depolarization is further augmented by the law which makes polarization depend reciprocally upon permeability, so that the equilibrium of the nerve-membrane system is completely upset, the polarization now decreasing to a minimum while the permeability increases to a maximum."[10]

If an external depolarizing agent is applied very slowly at the cathode, the permeability of the membrane decreases below normal, the polarization increases above normal or remains at normal, and the depolarizing agency fails to stimulate. But "if the external depolarizing agency is applied very rapidly the membrane does not have time to develop its compensating reaction, so that the threshold depolarization is reached and excitation results."[11]

We shall have to let this matter rest with one further quotation from Troland's paper. "There are undoubtedly certain physical *constants* which determine the processes of the nerve membrane. Among these are the threshold depolarization required to stimulate, the magnitude of the excitation or maximal depolarization, the rate of recovery of the membrane, etc. At the synapse these constants appear to suffer a radical change in magnitude, of such a character that all of the processes are retarded; the latent period is much longer, as is also the refractory phase, and if we suppose the synapse to have a phase of hyperexcitability this

[8] *Op. cit.*, 329.
[9] Troland, *op. cit.*, 331.
[10] *Op. cit.*, 333.
[11] *Op. cit.*, 334.

also is probably much prolonged. By a proper choice of the values of the various membrane constants the majority of the characteristic features of synaptic functions can be explained."[12]

The reader should refer to the original papers for evidence in support of these views. We have given no evidence justifying the theory, but have attempted merely to state it; and even this has been done in a very summary fashion.[13] However, we are now in a better position to make several concrete suggestions in regard to the learning process, the first being in terms of an alteration in the nerve-cell membrane.

III. Learning and the Nerve-Cell Membrane

We may now take up the learning process where we left it in *I.*, and attempt to explain how the association between B

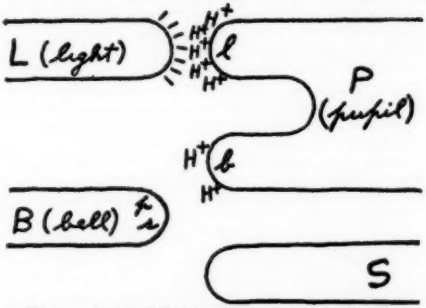

FIG. 3. A unit of the situation as represented in Fig. 2. L represents a portion of an end-brush in the path from an increased light-intensity to a contraction of the pupil. P represents part of a dendrite or cell-body in this same path. B represents a portion of an end-brush innervated by the bell-stimulus. The portion of P at *l* is close to L, and the part of P at *b* is close to B. The path from L to P continued in fairly good working order throughout the conditioned-reaction experiment described in the text. B would innervate P at the end but not at the beginning of the experiment. An impulse from B may or may not have passed over to a dendrite or cell-body S, calling out various reactions in the organism.

and P in Fig. 2 was formed. In Fig. 3 we have represented a still smaller unit of the situation as shown in Fig. 2. We shall continue to use the experiment on the conditioned pupillary reaction for illustrative purposes. While we make use of several principles included in the Nernst-Lillie theory, our suggestions regarding the physical basis of the conditioned response do not depend solely upon this theory. The Nernst-Lillie theory relates directly to only one of the four general groups of factors which we shall consider.

In Fig. 3, L represents a portion of an end-brush in the path from an increased light-intensity to a contraction of the pupil.

[12] *Op. cit.*, 344.

[13] For a general and historical discussion of nerves as conductors, cf. G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 127-144.

P stands for part of a dendrite or cell-body in the same path. B represents a portion of an end-brush innervated by the bell-stimulus. The portion of P at *l* is close to L, and the part of P at *b* is close to B. The path from L to P continued in fairly good working order throughout the experiment. An impulse from B may have passed over to a dendrite or cell-body S, thereby causing some reactions in the organism. The general significance of S has been discussed in *II.*, in connection with Fig. 2, and these considerations apply to Fig. 3 as well.

As explained above, B and P are close together at several points. In our illustrative experiment, P was innervated and made active by L 400 times. B was also active every time L and P were active. At the beginning of the experiment, an impulse would not pass from B to P; but after 400 repetitions of the training stimuli (L, P, and B active simultaneously) L could be omitted and B alone would innervate P. The compound stimulus, L with B, followed of course by activity in P, continued for about 1 sec. at each repetition. Now every time P was made active by L, the ionization process in P was increased. This caused more positive hydrogen ions to appear on the outside of P than would otherwise have been the case. While the processes described in *II.* were taking place in the synapse between L and P, the positive hydrogen ions appearing on the outside of membrane *b* were also being neutralized by a "spray of electrons" from B.[14] Now if B had not been active at all, *some* hydrogen ions would have been present on the outside of P, even on portions of P situated at a distance from *l*. The *simultaneous* activity of B and P, however, causes a considerable *increase* in the activity at *b*. Hydrogen ions are now neutralized in large quantities at *b* by electrons coming from B. Greater strains are set up in *b* by the stronger lines of force. The movement of ions through *b* would have been "leisurely" if B had not been functioning simultaneously with P, but when B and P are active simultaneously hydrogen ions move through membrane *b* in much larger numbers. The ions coming out of P,

[14]When an impulse is passing along a nerve, caused either by electrical, mechanical, chemical, or reflex stimulation, the point reached by the impulse is negative to all other parts of the nerve. This is the well-known "wave of negativity." Some free electrons are always present during stimulation; and the velocity of the nerve impulse and the action current are identical. The nerve always shows an action current when it is active, and the influences which alter the velocity or strength of a nervous impulse affect the velocity or strength of the action current in the same manner. Conduction along a nerve (or, rather, in each neurofibril) has been likened to that taking place in a "core conductor." (See W. H. Howell, *A Text-Book of Physiology*, 1918, 108; E. H. Starling, *Principles of Human Physiology*, 1920, 284. For a brief statement of the various influences which may modify the nerve impulse, see Howell, *op. cit.*, 113-116.)

through *b*, and toward B, knock the molecules of membrane *b* in position. They open up pathways in membrane *b*, and cause a rearrangement of the molecules. The molecules of membrane *b* may now be said to be *oriented*, so that electrons can "flow" through it more readily than before. The action of the whole system thus results in an increased permeability of membrane *b*, which is therefore left in a condition which is more favorable for the passage of an impulse from B to P than at the beginning of our experiment.

Perhaps it is unnecessary to call attention to the fact that the explanation just suggested does not follow point by point the theory of nerve-functions outlined in *II*. The theory developed by Nernst, Lillie, Lucas and Troland attempts to account for the passage of a nervous impulse *after* the association has been formed. Our task is to explain the *formation of the association* in the first place, granting that after it is formed the nerves may function as these writers describe. From some points of view, ours is the easier task.

There are several phenomena in nature similar to the one described above. For example, a piece of iron can be magnetized by rubbing it on a permanent bar magnet, and thus causing (in somewhat popular language) a rearrangement of its molecules. If you give this temporary magnet several sharp blows, the magnetism will be lost, because the arrangement of the electrical forces has been disturbed.

According to the theory of nerve-functions outlined in the previous Section, the passage of a nervous impulse from L to P is accompanied by an oscillation of the permeability and polarization of the membrane around P. Such an oscillation apparently affords even better conditions for a definite alteration in the structure of membrane *b*. This oscillation causes a change in the distribution of the forces which hold the membrane together. The change is further aided by the simultaneous activity of B, which, acting somewhat like a battery of machine guns, shoots more electrons through *p* in the direction of *b*, neutralizes more positive hydrogen ions at *b*, causes more hydrogen ions to pass through *b* from within P, and in general increases the chemical activity around *b*. As Troland suggests, the slight generation of heat may be neutralized by the "evaporation" of hydrogen ions. More carbon dioxide is also given off at *b* than would have been the case if B had not been active.

In the above discussion, we have said comparatively little about the rôle of S (Fig. 3), which represents the "secondary reactions" called out by the bell at the beginning of the experiment, including the dilatation of the pupil. We have seen that the dilating effect of the bell which was present at the beginning of the experiment was overcome by the training, and we do not have to consider it further at this time. We may, however, consider the other effects which were produced in the organism

by the bell-stimulus, such as change in heart-beat, breathing, etc., disregarding from now on the dilatation factor. These reactions are arbitrarily referred to as "secondary" because they are not particularly significant in the mechanisms we describe. The nervous pathways leading to them from B are designated by S in the figure. We are not absolutely certain whether S was present at all in the simple situation which we have been considering for the sake of clearness and for illustrative purposes; and, if present, we do not know whether an impulse was passing from B to S at any time during the experiment. In case S was absent or present and not functioning, we have nothing to explain. In case S was present and functioning, then the following considerations apply. There is apparently nothing in the situation which would *strengthen* the connection between B and S, and we have shown how the connection between B and *P* is strengthened. There is therefore a tendency for energy from B to be diverted from S to P. Since reflex movements called out by a sound-stimulus become adapted or fatigued very rapidly, we have some reason for believing that in many cases an impulse did not continue to pass from B to S for more than a few minutes. In some cases *both* synapses (between B and P and between B and S) may have been functioning at the end of the experiment. In other cases the connection between B and S may have been functioning at the end of the experiment but the synapse between B and P at no time during the experiment; and we are of course not concerned with this alternative, since no connection would have been formed between B and P. There are still other cases, however, where a connection *was* formed between B and P; and it should be remembered that there is a tendency for energy from B to be diverted from S to P. In *some* cases impulses passed from B to P and not from B to S, and these cases are significant from our point of view. The possibility of S being present and active is not necessarily a criticism of the processes which we have outlined above, or which are yet to be described.

While the above considerations seem to be a part-explanation for some forms of *temporary* modification, they may not be a very satisfactory explanation for many kinds of more or less *permanent* learning. The alteration in the structure of membrane *b*, resulting in a situation more favorable for the passage of an impulse from B to P, may be only *temporary* in nature, and membrane *b* may quickly revert to many of its old habits. A slight shock might cause the particles in *b* to drop back into their old positions. Although the molecules of membrane *b* are oscillating back and forth continually (heat or temperature quality), they may not leave the immediate vicinity, but act as if they were tied together by "strings." In the experiment

and general situation referred to above, there is a possibility that the "strings" holding the particles of membrane *b* in position were stretched considerably but not broken. Some shaking-up process might make membrane *b* revert to its old condition—just as iron filings which are scattered on a piece of paper in a magnetic field tend to rearrange themselves if the paper is tapped.

This suggestion in regard to the temporary alteration in structure of the cell-membrane is in harmony with certain external aspects of memory which are commonly observed. A very large percentage of the associations formed in ordinary life are *temporary* in nature. In the two experiments also which the writer carried out on the pupillary and eyelid reflexes, memory for the conditioned reactions persisted for only a short time. Many learned reactions in everyday life are as quickly forgotten, *e. g.*, a person's name, a telephone number, etc. In the ordinary process of reading, the number of reactions which are learned, and forgotten before 10 min. have passed, mounts up into the thousands if the process is continued for just a few hours. The very sudden drop in the curve of forgetting for material that is just barely learned also illustrates the same fact.

There are no *qualitative* differences between the modification of a reflex and the kind of learning which especially involves the "association areas" of the cerebrum. Learning a person's name involves more particularly the *connecting* process, and does not *oppose* well-established reaction-tendencies. An alteration in reflex behavior, on the other hand, generally involves a much greater mass of bodily tissue; it is of a more drastic nature, and is accomplished only by *opposing* integrated neuro-physiological responses; that is to say, by opposing well-established tendencies which are in excellent working order and nearly always ready for business. The physical changes involved in associating the name "John Long" with a rather tall individual involve especially the *connecting* process, because both "John" and "Long" are already familiar, and the appearance of this individual may suggest past experiences to us. All that is necessary in forming this bond is to *connect* two things, either one of which is working smoothly. The "association areas" of the cerebrum afford ample opportunity for such connections being formed. The process is mainly one of shunting nervous impulses in a new direction with practically an open road in front, or of the transference of a comparatively small amount of energy where the distance to be moved is slight. In modifying a reflex response, however, we must *unlearn* a much larger number of connections. Some of these have been functioning since birth, and in some cases before birth. They are well "oiled" and very efficient from the standpoint of pure dynamics.

If we take account of these considerations, it is not surprising that an association may be formed between the pupillary reflex and an originally non-effective stimulus only after prolonged training, whereas a person's name may be learned in a few seconds.

The physical mechanisms discussed in this Section are related to most of the other physical processes which will be described in the three following Sections. The change in membrane *b* is only one of the factors at work in the association process.

IV. Learning and a Gross Movement of Nervous Elements

In the situation represented in Fig. 3, there is a possibility that the nervous elements B and P may actually move toward each other. There are no facts in neurology which militate against the possibility of such a movement, and there are several reasons why such movements probably take place. End-brushes and dendrites are composed of very fine branches practically swimming in an aqueous solution, and it is to be expected that these fine branches would not be entirely stationary. Our explanation of the possible cause of such a movement is as follows.

The electrolytic dissociation process which is at work in the situation represented by Fig. 3 causes positively charged particles in P to migrate toward *b* and *l*, and negatively charged particles in P to move away from *b* and *l*. However, not all of the negatively charged particles in P move away from *b* and *l*; some remain, and assist in forming the electrical double layer referred to in *II*. The situation represented in Fig. 3, however, results in an *increased* movement of positive hydrogen ions toward *b*, which is due to electrons being delivered or freed at this point, partly because of the action of B. The electrical charge on the outside of *b*, as we have already seen, is *positive* in character. Since *b* is comparatively close to the principal mass of the cell-body P, the ionization process in cell-body P can supply *b* with a comparatively large number of positive hydrogen ions. Membrane *p*, on the other hand, is generally situated out near the end of a fine process and at some distance from the cell body of which B is the end-brush. Although the ionization process in the neurone of which B is a part may cause some positive hydrogen ions to appear on the outside of *p*, these hydrogen ions cannot be delivered at this point in as large quantities as at *b*, just across the way. An electrical wave of stimulation in B is also of greater general consequence than one of equal strength in P, because of the difference in volume and also because the hydrogen ions on the outside of *p* are more quickly neutralized than those on the outside of *b*. The free electrons

in B are probably more concentrated than in P. To use rather popular language, it may be suggested that the free electrons in B are in a regular stampede to get through *p* and to get at the hydrogen ions on *b*. The attractive forces present between the positive hydrogen ions passing out of P through *b* and the electrons passing out of B through *p* tend to move *p* toward *b*, and *b* toward *p*. Since B is smaller than P, and also because it is on the end of the very fine axone and not attached to a stocky branch, B will probably move more than P.[15]

The considerations regarding S which were stated in *III.* apply here as well. In the situation which we have described there may be some attractive force between B and S, but there is nothing in the situation which would increase this stress, and the attractive force between B and P *is* apparently increased. If B is suspended somewhere between P and S, its position at any given time will depend upon the relative attractive forces exerted by P and S. Having once moved to P, branch B might conceivably stick there just as a tack will stick to either one of two bar magnets, depending upon the relative position of the tack and the two bar magnets in space. Even if B moves only part of the way toward P, this movement will aid the learning process, since a decreased distance between B and P naturally increases the probability of an impulse passing from B to P.

This explanation of a movement of B and P toward each other may be a part explanation for that type of learning where the association is quickly made and also easily retained. This process also affords a part explanation for sudden forgetting due to *shock*. In such cases it is only natural that those learned reactions which involve the cerebrum especially would be more disturbed than certain well-established reflexes which generally involve fewer successive synapses. The effect of shock on loss of memory, loss of consciousness, etc., is more readily understood by reference to the physical elements described above. Nervous elements are doubtless jarred loose and things are forgotten in somewhat the same general way that a shock jars lead out of a storage battery. The reaction tendencies are changed and forgetting takes place in both cases.[16]

We do not wish to exaggerate the importance of a gross movement of B and P toward each other. Other factors must operate at the same time. This is just another factor at work in the association process.

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[15]See H. H. Bawden, A Digest and a Criticism of the Data Upon which is Based the Theory of Amoeboid Movements of the Neurone, *J. of Compar. Neurol.*, 10, 1900, 243-264; G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 288-292.

[16]W. Ostwald has attempted a chemical theory of memory in his *Vorlesungen über Naturphilosophie*, 1902.

V. Learning and the Substance Within the Cell

We have now considered two groups of physical factors which are involved in the conditioned response. First, there is a temporary change in the internal structure of membrane *b* (Fig. 3); and, secondly, there is a gross movement of B and P toward each other. These two groups of factors do not operate independently, and they are also not to be considered as alternative or exclusive explanations, since both are probably at work in all learning. We have next to consider a third factor, namely, the rearrangement of substances within the cell body itself. We ask the reader's indulgence while we consider first a few facts drawn principally from colloid chemistry, the "promised land of the biological scientist."

Colloid chemistry is principally concerned with the properties of a physicochemical state, rather than with the properties of any single group of substances.[17] If the particles in the disperse system are of microscopic or submicroscopic dimensions, the surface of contact between this phase and its neighbors will be very large, the surface forces will be very apparent, and the dispersoid will have varied and peculiar properties. In the dispersed condition, the vapor pressure, chemical activity and many other properties are different from those obtaining in the form of large aggregates. There is no sharp line of demarcation, however, between colloids and crystalloids.[18]

The adsorption coefficients of H^+ and OH^- ions are very high; and acids are generally better adsorbed than their salts. Colloids are especially well adsorbed and an ion of higher valence will generally be adsorbed more strongly than one of lower valence. In positive adsorption there is generally a fixation of the colloid or of the dispersed material upon the solid or other adsorbing surface, and this increase in concentration may cause an increase in the velocity of some of the reactions of the adsorbed substance.

In the electrolytic dissociation of an acid the charged atom attaches to itself several water molecules. Ions are generally made up of such electrically charged complexes. Colloidal particles may collect around these ions, the ions may be adsorbed by the colloidal particles, or the colloid itself may become ionized. When colloidal particles are electrically charged, the particles behave like ions and migrate through the solution under the influence of a potential difference. The particles in a colloidal solution, however, may be of varying size and they may also carry varying charges of electricity. An indifferent colloid in an acid solution will adsorb H^+ ions and become positively charged with respect to the fluid; while in an alkaline solution the colloid will become negatively charged with respect to the fluid medium. Negatively charged colloidal particles attract foreign crystalloid cations, and repel anions. Colloidal cations behave in the reverse way.

Water is prominent among the constituents of *protoplasm*; and many substances are negatively charged while others are positively charged with respect to water. The movement of colloidal particles in water may obviously be influenced by an electric current or by the addition of electro-

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[17]See E. Hatschek, *An Introduction to the Physics and Chemistry of Colloids*, 1916; W. W. Taylor, *The Chemistry of Colloids and Some Technical Applications*, 1915. The first-named is an elementary treatise.

[18]For some evidence favoring the position that the stoichiometrical laws of classical chemistry apply to colloids as well, the reader is referred to J. Loeb, *Proteins and the Theory of Colloidal Behavior*, 1922.

lytes. The dispersed phase itself may have acid or basic properties, in which case it will yield colloidal anions or cations respectively in pure water. Electrolytes are very common in living cells and the unequal distribution or concentration of ions accounts for differences in potential on the two sides of a membrane or diaphragm and between different parts of tissue. Various substances are carried to these cells in the lymph and blood, and these substances pass in and out of the cell membrane. The addition of alkalies will increase the negative charge if the diaphragm is negative. Increase in hydrogen ion concentration will tend to make such a diaphragm positive. In the case of a positive diaphragm, however, the addition of alkalies will decrease the positive charge, and increase in hydrogen ion concentration will tend to make such a diaphragm more positive. While differences of potential may develop between the aqueous and non-aqueous phases of these colloidal solutions, most animal membranes are the seat of electromotive forces only in faintly alkaline or faintly acid water.

The *structure of protoplasm* has received much attention from biological chemists, and a good many facts have been definitely established. Protoplasm may exist in all states from a liquid to a solid, and all of the complex molecules which compose it are colloidal in character.[19] In fact, all life processes take place in a colloidal system.[20] Protoplasm consists of a non-aqueous phase dispersed in an aqueous phase, and is a loose combination of proteins and lipoids.

The *nerve cell* is made up largely of proteins, while the nerve-cell sheath is composed principally of lipoids which behave quite differently toward acids and salts. Many *proteins* show the phenomenon of coagulation, which may be brought about by a rise in temperature or by chemical means. The adsorption phenomena have been studied most carefully in the case of albumin which has the character of a weak acid and a weak base, with the acid character more or less in excess. Proteins may be strongly adsorbed or they may be strongly adsorbent. There is an interesting parallelism between acid albumin and alkali albumin. When an *acid* is added to amphoteric albumin, the albumin migrates to the cathode, loses its coagulability by heat, its internal friction is increased, and its surface tension is diminished. When an *alkali* is added to amphoteric albumin, the albumin migrates to the anode, it is not coagulable by heat, its viscosity is increased, and its surface tension is diminished. Albumin ions are responsible for the increase in internal friction which has been observed.

The *functioning of nerves*, like that of muscles, is associated with a certain condition of swelling. Nerves lose their irritability when placed in solutions of non-conductors, and recover their irritability when placed in physiological salt solutions.[21] The *insulating sheath of nerve cells* is formed largely from *lipoids*, lecithin ($C_{44}H_{90}NPO_9$) and cholesterin ($C_{25}H_{44}O$).[22] Lecithins and cholesterin are widely distributed throughout the animal organism, and are of considerable biological importance.[23] They play an important rôle in the adjustment of metabolism between cells and their surrounding media. "Lecithin . . . forms an emulsion with water of its own accord; indeed like a protein it swells up in water into a turbid

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[19]See C. M. Child, *Senescence and Rejuvenescence*, 1915, 13-33.

[20]See L. Rhumbler, Das Protoplasma als physikalisches System, *Ergeb. d. Physiol.*, 14, 1914, 474-617.

[21]See H. Bechhold, *Colloids in Biology and Medicine* (trans by J. G. M. Bullowa), 1919, 354. Also C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906, 196-199.

[22]See G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 117-126, for a general account.

[23]Cf. S. Hatai, The Growth of Body and Organs in Albino Rats Fed with a Lipoid-Free Ration, *Anat. Rec.*, 9, 1915, 1.

colloidal solution, without dissolving. It may be said to occupy a place, in respect to its colloidal properties, between the emulsifiable fats and the hydrophile colloids, closely approaching the latter . . . Lecithin acts towards colloids and suspensions (ferric hydroxid, mastic suspension) like any other colloid which migrates to the anode. Similarly charged colloids cause no precipitation. .; oppositely charged colloids produce flocculation in suitable mixtures (ferric-oxid hydrosol). . . .*Cholesterin*, according to the investigations of O. Porges and E. Neubauer, is a hydrophobe colloid. Its aqueous emulsion behaves like a mastic suspension in the presence of a large variety of salts. The same is true for its behavior with other colloids. In neutral solution it is precipitated by certain proportions of albumin and saponin. Lecithin may act as a protective colloid for cholesterin."[24]

Althouth no membrane can be directly observed in the case of many animal cells, the cell-pellicle nevertheless has special properties. Lecithin and cholesterin collect in this membrane, and, among other things, change its surface tension. The thickness of the coherent pellicle (lecithin, etc.) may be as small as from 0.3 to 7.0$\mu\mu$. It may be invisible under the microscope and still fulfill its functions. It would be incorrect to consider the cell-membrane as strictly semi-permeable or stable in constitution, since it changes from time to time along with other parts of the cell. It may grow in one direction or in the other, or become thicker or thinner.[25]

There is much evidence from various sources that the transformation of energy in all kinds of matter, "dead" as well as living, is closely associated with electrical phenomena. Starling writes that "every beat of the heart, every twitch of a muscle, every state of secretion of a gland, is associated . . . .with electrical changes. . . .The uniform character of the electric response in different kinds of tissues suggests that an accurate knowledge of the changes in the distribution of charged ions responsible for the response ought to throw important light on the intimate nature of excitation generally."[26]

Returning now to the situation represented by Fig. 3, we should keep three general facts in mind: first, that an impulse passed from L to P at the beginning of our conditioned reaction experiment, and stimulated P; secondly, that an impulse *would not* pass from B to P at the beginning of the experiment; and, thirdly, that an impulse *would* pass from B to P after some training in the experiment had been given. While the system represented by Fig. 3 is active and while a "negative variation of the current of rest" is present on the outside of P, positively charged colloidal particles in cell-body P not only move toward the original source of stimulation, L, but also toward the new source of negative variation, B. When these colloidal particles reach membrane *b*, their positive charges are neutralized by the flow of electrons from B, and the colloidal substances are

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[24]Bechhold, *op. cit.*, 140-141. Cf. O. Porges and E. Neubauer, Physikalisch-chemische Untersuchungen über das Lezithin und Cholesterin, 2 and 3, *Z. f. Chemie u. Industrie d. Kolloide*, 5, 1909, 193-197.

[25]For an argument against the earlier views concerning osmosis and strictly semi-permeable membranes, the reader is referred to M. H. Fischer, *Oedema and Nephritis*, 1915, especially 3-34, 136-142, 153-164. Many problems of general interest are discussed in this work in addition to the strictly medical material.

[26]E. H. Starling, *Principles of Human Physiology*, 1920, 169.

deposited on the membrane, thereby forming a layer. This action increases the *concentration* of substances around *b*, probably improves the *conductance* in this region, and results in a situation which is more favorable for the passage of an impulse from B to P.

Various substances in aqueous solution are passing in and out of P, fulfilling the general nutritive functions of the cell. The amount of substance which will pass through membrane *b* depends upon a variety of factors, and among these is the nature of the solutions on the two sides of the membrane. Since the concentration of the dissolved substances is increased very markedly and hence the concentration of the solvent on the inside of *b* is diminished, a relatively larger number of the molecules of the solvent on the *outside* of the membrane will strike it in a given time, and because of the general mechanism of osmosis more material will pass into P through *b* than in the reverse direction. Although the osmotic pressure of colloidal solutions is low, it increases with the dispersion. With a very thin membrane this movement into P through *b* may also be influenced by the attractive forces between the substances on the inside and outside of membrane *b*. The movement will be mostly *into* P, since the particles on the outside of *b* are smaller. The meshes of the colloidal membrane may also be opened or closed, and this will naturally affect the diffusion.[27] The operation of the above factors will result in a *swelling* of P around *b*. (This does not necessarily mean that the *pressure* in P will be increased.) When P swells at *b*, B and P are brought closer together; and this improves the chances of an impulse passing from B to P.

Consider the effect of an increased hydrogen ion concentration on the swelling of the colloidal particles in P, especially at *b*. When the system represented by Fig. 3 is active, hydrogen ions are produced in P at *b* in larger numbers. It is well-known that a variation in the concentration of electrolytes within biological limits can cause a considerable variation in the water content. The presence of acids and alkalies increases tremendously the amount of water adsorbed by certain substances. Very dilute acids increase swelling, but when concentrated they have the reverse effect. "The influence of the hydrogen ion is so enormously great that the presence of even such a 'weak' acid as carbonic acid brings about a distinct increase in swelling."[28] Swelling depends upon the influence of the H-ions *minus* the

[27]For a general résumé of membrane-phenomena, the reader is referred to H. Zangger, Ueber Membranen und Membranfunktionen, *Ergeb. d. Physiol.*, 7, 1908, 99-160.

[28]W. Ostwald, *An Introduction to Theoretical and Applied Colloid Chemistry* (trans. by M. H. Fischer), 1915, 161.

influence of the anions of the acid under consideration. There is an antagonism between anions and cations. Swelling in acids and alkalies is also considerably decreased by the presence of neutral salts. As stated in *III.*, the passage of an impulse from L to P ordinarily results in the liberation of a certain number of hydrogen ions in cell-body P, *but the simultaneous activity of B causes a larger number of hydrogen ions to be liberated around b than would otherwise have been the case.* Water is drawn *from* the space between *p* and *b into* that portion of P around *b*. This naturally causes a swelling in P around *b*, decreases the distance between B and P, and favors the passage of an impulse from B to P. There is also a tendency towards an increase in free surface with ionization and swelling, and this causes certain chemical reactions, especially hydrolytic cleavages, to take place more rapidly.

It is not difficult to understand that the chemical changes just enumerated have a more or less *permanent* character. After the swelling has taken place in nerve-cell P the heat produced by this swelling coagulates certain colloids. Colloids of opposite sign may also coagulate the protein substance of the cell. Electrolytes are known to coagulate certain colloidal solutions, and colloids of opposite sign may coagulate each other. While the coagulation of *suspensoids* by electrolytes is generally a reversible reaction, the coagulation of *emulsoids* is frequently irreversible or reversed only with difficulty. Electrolytes commonly cause flocculation in colloids. Small particles come together to form larger complexes. In the case of positively charged colloids, the presence of anions or negatively charged colloids will neutralize or lessen the electric charges, so that the particles will be freer to unite. The H^+ ion has a very powerful flocculating action upon colloids which migrate to the cathode. The reverse of flocculation is peptization, when the particles go back in solution. Peptization may be caused by an excess of electrolyte which renews the electric charge.

Other complicated factors are at work in addition to those described above, and among these may be mentioned the chemical reactions of colloids which are not due to the adsorption phenomenon; the interaction between different colloids; the relations between stability, solubility, viscosity, and conductance; the effect of an adsorbed ion on solubility; the action of protective colloids; the effect of heat and pressure on stability; the effect of a change in temperature, pressure, etc., upon the physicochemical equilibrium; the precipitation of insoluble substances; and the rôle of colloids and various ions in catalysis. The factors which we have mentioned, however, are fairly well defined and are clearly at work in the situation represented by Fig. 3. Our suggestions also seem to reinforce and

harmonize very well with each other. While points of false equilibrium may be established by a variety of causes, there is ample opportunity for the effects described above persisting for a while because of the numerous stabilizing forces at work.

In the above discussion, we have referred to the nerve-cell body as if it were a more or less homogeneous mass of protoplasm, and we have said very little about the differentiated structures which are present in the cell, such as the nucleus, nucleoplasm, chromatin, cytoplasm, neurofibrils, perifibrillar substance, etc. Some of these structures are chiefly nutritive in function, and concern us only indirectly. While different types of nerve cells apparently account for the differences in function, and *vice versa*, much work remains to be done on this subject before we can speak with certainty in regard to the correlations between different types of nerve cells and learning phenomena. There is a certain type of so-called association neurone in the cerebrum, but only a few very general facts can be stated concerning the relation between its structure and function.[29] The nerve-cell membrane has also been considered in the above discussion as if it were composed of a single layer, whereas what we have said refers especially to the superficial layer. Comparatively little is known about the difference in chemical constitution in the differentiated structures of the nerve cell.[30] The nucleus of most animal cells seems to be a complex of hydrosols, containing large particles, and it is poor in water.[31] The chromatin substance of the nucleus seems to have pronounced acid properties.

The complex factors mentioned above afford a plausible explanation for the very great differences observed in the general *impressibility* and *retentiveness* of different individuals.[32] The presence of certain chemicals in different proportions could easily account for a variety of individual differences, and the glands are already being considered in this connection. The facts which have to do with the *ageing* of colloids and the external aspects of memory also harmonize very well with each other. Crystalloids often retain their physical properties indefinitely, but colloids are metastable systems and change after a lapse of time. Age brings about a decrease in the swelling capacity and elasticity of membranes and also causes shrinking. Diffusion takes place more readily through young and fresh membranes

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[29]See E. F. Malone, Recognition of Members of the Somatic Motor Chain of Nerve Cells by Means of a Fundamental Type of Cell Structure, *Anat. Record*, 7, 1913, 67-82; The Nucleus Cordiacus Nervi Vagi and the Three Distinct Types of Nerve Cells Which Innervate the Three Different Types of Muscle, *Amer. J. of Anat.*, 15, 1913-1914, 121-130; G. W. Barthelmez, Mauthner's Cell and the Nucleus Motorius Tegmenti, *J. of Compar. Neurol.*, 25, 1915, 87-128; C. J. Herrick, *An Introduction to Neurology*, 1918, 105-112.

[30]See F. R. Bailey, *A Text-Book of Histology*, 1920, 130-149; W. H. Howell, *A Text-Book of Physiology*, 1918, 129-139; E. H. Starling, *Principles of Human Physiology*, 1920, 300-314.

[31]Cf. A. Kossel, Ueber die chemische Beschaffenheit des Zellkerns, *Münchener medizinische Wochenschrift*, 58, 1911, part 1, 65-69.

[32]Cf. A. F. Tredgold, *Mental Deficiency*, 1920, 77-83; K. S. Lashley, The Effects of Strychnine and Caffeine upon the Rate of Learning, *Psychobiology*, 1, 1917, 141-169.

than through old ones. Since there is a general reduction in metabolism it is not difficult to understand why a young person can learn new things more readily than an old person. The old person finds it difficult to produce a very radical change in his "apperception mass" partly because those portions of his nervous system corresponding to *b* in Fig. 3 are not so elastic, and diffusion does not take place so readily through them. The water content of the body also decreases with age.[33] Since time brings about a shrinkage in the cell membrane, the operation of the recency factor in memory is easily explained. If sufficient time elapses both B and P in Fig. 3 will shrink and the association will probably be weakened.

VI. Learning and Surface Tension

The superficial layer of all biological cells is in a so-called state of tension, and is said to exert a pressure on the substances within the cell. The surface tension[34] tends to diminish the surface, and to compress the substances within the cell. Many changes in the form of living substances are due ultimately to these forces.

Strictly speaking, osmotic pressure is manifested completely only in the case of semi-permeable membranes, and it may be counter-balanced by swelling in some cases. The cells of the organism retain their general form; and the cell content is more subject to swelling than the surrounding membrane. A diminished surface tension at any point on the bounding surface of the nerve cell will cause a bulging at this point. "It must be remembered that the passage of a current through a membrane impermeable to one or other ion in the surrounding fluid will cause an accumulation of the ion at the surface of the membrane, so that this will become polarized. Such an accumulation at any surface will naturally alter the properties of the surface including its surface tension.[35] The construction of the capillary electrometer depends on this fact." Surface tension is very sensitive to a variety of factors; slight traces of various substances, especially colloids, exert a marked influence upon it.

An increase in concentration on the surface film of a true solution (*i. e.*, adsorption) seems to be accompanied by a lowering of the surface tension (Freundlich).[36] Bancroft, in discussing a theory of peptization, writes that "we find experimentally that certain solutions will peptize or disintegrate certain precipitates so that we get colloidal solutions. This is always the result of adsorption. Theoretically there are three possibilities. If an adsorbed film has a low surface tension on the water side and a high one on the other side, it will tend to scrunch up and to peptize the solid as internal phase. If the reverse is the case, the solid will tend to form the external phase. If the two surface tensions are practically equal, neither will prevail. . .If we adopt Freundlich's view that adsorption always

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[33]Cf. C. M. Child, *Senescence and Rejuvenescence*, 1915, 283-284; S. Paton, *Human Behavior*, 1922, 311-313.

[34]See E. W. Washburn, *An Introduction to the Principles of Physical Chemistry*, 1915, 42; H. Bechhold, *op. cit.*, 1919, 13-40; D. W. Thompson, *On Growth and Form* 1917, 205-209.

[35]Starling, *op. cit.*, 172.

[36]Cf. D. W. Thompson, *op. cit.*, 277-281.

lowers the surface tension, a theory of peptization follows at once. We may have peptization by a liquid, by a non-electrolyte, by an adsorbed ion, by a salt, or by a peptized colloid. When a liquid is adsorbed by a solid, it will tend to peptize it and in some cases will do so."[37] Electrolytes which have a readily adsorbed *anion* lower the surface tension of the water phase in colloidal solutions, and increase the permeability of the system to water, but electrolytes which have a readily adsorbed *cation* produce the reverse effect.[38]

In the situation represented by Fig. 3, there is probably a slight compression of membrane *b*, resulting from the presence of the electrical double layer. Although very slight, this pressure according to Lillie is sufficient to alter the permeability. Dissolved substances have different effects on surface tension. In general, salts and strong bases in the solution *increase* the surface tension, whereas nitric, hydrochloric, and hydrobromic acid *decrease* it.

It is clear from the above considerations that in the situation represented in Fig. 3 there will be a *decrease* in surface tension at *b*, and probably also at *p*. The decrease in surface tension may be due to a polarization of the membrane (or to an increase in concentration on the surface film), or to a variety of dissolved substances in solution. This decrease in surface tension will cause a bulging at the point affected, most probably at *b*. With *p* and *b* closer together, a situation will result which is more favorable for the passage of an impulse from B to P.

The relation between surface tension and osmotic phenomena is so complicated and involved that we cannot enter into a discussion of this question.[39] Although there are many things about surface tension and osmotic phenomena which are but poorly understood, all of the general properties which we have discussed are agreed upon fairly well by specialists in this field, and they show a relation between surface tension and the association process.

VII. The Complete Process in Association

We may now attempt to visualize the situation represented by Fig. 3 in *three dimensions*. There are *numerous* points of contact between L and P, since the light-stimulus changed the size of the pupil at all times during the conditioned-reaction experiment; there are probably *several* points of contact between B and S, because of the "secondary reactions" called out; and a *few* between B and P, since conditioned reactions were obtained. The ease with which an association can be formed in this situation will depend very largely upon the number and

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[37]W. D. Bancroft, *Applied Colloid Chemistry: General Theory*, 1920, 166-167.

[38]Cf. Bechhold, *op. cit.*, 38-40.

[39]Refer to A. Findlay, *Osmotic Pressure*, 1919.

arrangement of these neural elements. There may be several different combinations of the neural elements represented by the Fig. Millions of neurones are involved even in some of the simplest movements of the organism; and we have a large number of possibilities to consider. This situation involves not only the fairly well-defined processes which we have described, but other factors also which, although present and doubtless active in the association process, do not invalidate the definite factors which we have described, but merely complicate them to some extent. For example, the nervous elements and processes are influenced by the substances carried in the lymph and the blood, by temperature, etc. The blood supply of nerves, however, is very slight.

It is quite possible that some overt movement which we are attempting to associate with a given stimulus will not take place although a few thousand systems or situations represented by Fig. 3 are operating as described. In the case of the eyelid response used by the writer in some experiments,[40] it is clear that an eyelid reaction is *very large* when compared with the unit-situation which we have been considering. In some cases overt movements were not obtained in our experiment. To explain these satisfactorily we have furthermore to take account of the "all-or-none" character of neurophysiological mechanisms. In such an experiment, some tendency is doubtless left in the nervous system, even though the reaction in question is not evoked; and the nervous system is probably always affected whether a *visible movement* takes place or not. Failure to obtain a conditioned knee-jerk reaction is no proof that a tendency in this direction has not been left in the nervous system.

Some psychologists may have difficulty in harmonizing these external aspects of association and memory. Why, for example, is it easy to learn a person's name but hard to establish a conditioned knee-jerk reaction? We believe that if sufficient data were at hand in regard to the operation of the physical factors involved this difficulty would disappear. Considerable help can be obtained if, when considering any phase of the association process, we consider especially the following factors: (1) the *physical mass* to be moved, (2) the extent to which the reaction is *partly learned* already, (3) whether or not the new movement *opposes* reaction-tendencies which are already well established, (4) the "all-or-none" character of neurophysiological mechanisms,[41] and (5) the structural relations of various

[40]The Conditioned Eyelid Reaction, *J. Exper. Psychol.*, 5, 1922, 153-196; A Note on the Conditioned Eyelid Reaction, *ibid.*, 6, 1923, 82-83.

[41]Evidence favoring the all-or-none law has been accumulating steadily during the past few years. See E. D. Adrian and A. Forbes, The All-or-Nothing Response of Sensory Nerve Fibres, *J. of Physiol.*, 56, 1922, 301-329.

elements in the nervous system. If all of these factors are considered, we should have no difficulty even now in understanding most of the important phenomena of association and memory.

VIII. Summary

We have now completed our survey of the physical factors involved in forming a connection between B and P in Fig. 3. We have attempted simply to outline the various factors which are at work in this system, and we have gone into detail only where the facts seemed to justify it. All that we can say in regard to a few of these factors is that they are very probably present and operating, but most of the factors described seem to be present and operating beyond reasonable doubt. It has been necessary to make comparatively few assumptions which are not well supported by scientific evidence. The complete picture sketched represents a *complex* situation, and we are convinced that future investigators of this problem should avoid looking for any clear-cut, simple, or *neat* explanation of association and learning,—which has been the custom in the past.[42]

We have attempted to show how the action of the system represented by Fig. 3 causes a change in the structure of membrane *b*, which in turn produces a situation more favorable for the passage of a nervous impulse from B to P. This orientation of the molecules of membrane *b* is probably only temporary in nature, but partly accounts for certain temporary changes in reaction-tendencies which are commonly observed.

There is some reason for believing that B and P (in Fig. 3) may move toward each other when the system is active. Such a gross movement of nervous elements affords a partial explanation of the rapid formation of some associations; and considerations regarding such movements have a bearing on the effect of shock on loss of memory.

Facts gathered from colloid chemistry establish a definite relation between the action of the system represented by Fig. 3 and certain swelling phenomena in the nerve cell. The action of the system results in a swelling in P, around *b*, which is due to certain osmotic phenomena in this region and to the effect of an increase in hydrogen ion concentration on the swelling phenomena in the cell. This swelling, and the increase in the conductance of electric currents in the region of *b*, result in a situation which is more favorable for the passage of an impulse from B to P. Other chemical considerations show how these changes may have a more or less permanent character. We

[42]Cf. H. Cason, The Concept of Backward Association, this JOURNAL, 35, 1924, 217-221; Criticisms of the Laws of Exercise and Effect, *Psychol. Rev.*, 31, 1924, 397-417.

have apparently worked out a partial explanation for many of the observed differences in impressibility and retentiveness of various individuals. The facts which have to do with the ageing of colloids harmonize well with the external aspects of memory. The action of the system decreases the surface tension at *b*, causes a bulging at this point, and thereby assists in the formation of a connection between B and P.

Throughout the above discussion, we have emphasized the combined action of *all four factors*, arbitrarily grouped: the change in structure of membrane *b*, the gross movement of B and P toward each other, the rearrangement of substances within cell body P, and the effect of a change in surface tension at *b*. These partial explanations should not be considered as exclusive or alternative explanations. We have attempted to emphasize the combined action of the whole system, rather than the action of any single factor which cannot function alone.

In conclusion, it should be stated that we have only attempted to treat a very limited phase of the learning problem, *i. e.*, the part which is related particularly to nerve physiology, physics, and chemistry. Learning and habit, however, cannot be adequately expressed *merely* as a function of electrical attractions and repulsions. The logical conclusion of such a crude mechanical view would be as materialistic as it is incomplete, and would deny all human characteristics with which we are personally acquainted. The *biological* point of view places too much emphasis on physiological mechanics and native factors, and is somewhat limited and narrow in its scope. It exaggerates the points of similarity between the meagre learning capacities of the brutes and those of man. Man's knowledge, skill, character, temperament, customs and institutions, however, bear no resemblance whatever to those of a lower animal. Unjustified emphasis on the structure and function of man considered from the narrow biological point of view has frequently led to the most absurd conclusions. It is necessary to take proper account of our central core of conscious reactions, our memories, motives, hopes, fears, and so on. The complete picture will have to include social considerations: the response should be considered in immediate relation with the stimulus. Only by considering the environmental influences can we form anything like an adequate conception of learning and habit-formation, and only in this way can we understand how man through long periods of time has acquired the varied characteristics which he now possesses.

ATONIC ENDINGS IN MELODIES[1]

By Paul R. Farnsworth

Introduction

The tonic effect[2] is a well-known fact of musical aesthetics. As an example of this, consider a melody composed only of the notes c and g. The vibration-frequency relationship here is as 2 to 3. The majority of listeners to this simple melody will prefer its ending on c rather than g,—that is, they will prefer ending on the ratio 2, the tonic. On the other hand, when c and f (frequency 3 to 4) constitute the melody, the ending-preference will now be f. This again shows the preference for ending on 2 (in this case 4 with the powers of 2 extracted).

With the exception of Schoenberg and other ultra-moderns, composers have adhered quite generally to this principle of closing a piece on its key-note which is generally also the tonic. In the key of c, we end on c; in the key of g, on g, etc. A feeling of rest or finality is conceded to accompany this ending. This decided preference for ending on the pure power of 2 has been shown experimentally by Meyer.[3] According to his results, the ratio symbol 2 was preferred over 3 as an ending in 86 instances, and 3 was preferred over 2 in only 26 instances; or, in terms of percent., the ratio symbol 2 was preferred over 3 as an ending in the proportion 77 : 23.

In a composition in which the pure power of 2 does not exist, it is supposed that one ending exhibits as much finality as any other. As it seemed to the writer that this assumption has been accepted without much actual investigation, the following experimental work was undertaken.

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[1]From the Department of Psychology of Ohio State University.

[2]M. F. Meyer (*Psych. Rev.*, 7, 1900, 247) states the general law as follows: "One of the tones being a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody."

As this notation may be new to some readers, let me again quote from the same article (252). "All pure powers of 2, as 1, 2, 4, 8, 16, 32, etc., will be represented equally by 2. All composite numbers containing a power of 2 will be represented without the power of 2. So I always shall write simply 5 instead of any of the numbers 5, 10, 20, 40, etc., 9 instead of 9, 18, 36, 72, etc."

These ratios with the pure powers of 2 extracted will be designated in this paper as *ratio symbols*.

[3]M. F. Meyer, this Journal, 14, 1903, 459.

Experimentation

Three tones, g, e and b♭ (key of c) were chosen as representing the ratio symbols 3, 5, and 7, in which the pure power of 2 is absent. The corresponding musical notation, vibration-ratios, and actual vibration-rates, are here listed.

Musical Notation	Ratio Symbol	Vib. Ratio	Vib. Rate
g	3	6	96.89 d.v.
g′	3	12	193.77 d.v.
e′	5	10	162.94 d.v.
b♭	7	7	115.22 d.v.
b♭′	7	14	230.43 d.v.

These tones were played successively on a reed-organ. The three tones were so arranged that in one group g (3) was the last heard[4], in a second group e (5) was the last, and in a third group b♭ (7) was the final tone given. The groups were sounded in a random order, and were judged by the method of paired comparisons, as is shown in the following experimental programme.

Experimental Programme

Three groups of students in elementary psychology acted as *S*s.

Test A	58 Students
Test B	62 Students
Test C	62 Students (same group as B)

Thirty judgments were made by every student, as follows.

Ten judgments with 3 and 5 paired
- Five judgments with 3 first and 5 last
- Five judgments with 5 first and 3 last

Ten judgments with 3 and 7 paired
- Five judgments with 3 first and 7 last
- Five judgments with 7 first and 3 last

Ten judgments with 5 and 7 paired
- Five judgments with 5 first and 7 last
- Five judgments with 7 first and 5 last

The instructions were as follows: "You will hear two series of three tones each which you are to compare. Which series sounds more finished, more complete, more at rest? If it is the first, mark 'one' on your paper; if it is the second, mark 'two'". The instructions were repeated so that every *S* understood what was expected of him.

Tests A and B

In Tests A and B, the tones were heard with a falling inflection. The three different ending-possibilities are listed under the titles, Successions I, II, and III. Succession I was paired with II, II with III, and I with III.

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[4]In this article, the number in parenthesis in the text will refer to the ratiosymbol.

As an example, let us take the case in which Succession II (b♭ g e) was paired with Succession III (g e b♭). In the first case, b♭ was the highest and e the lowest note in the succession, in the second case g was the highest and b♭ the lowest. In other words, the comparison here was between ending on e (5) and on b♭ (7). The same experiment was given to two groups,—58 in Test A and 62 in Test B.

Ratio Symbol				Vib. Ratio in Time-Order Presented		
				1st	2nd	3rd
			Succession I	e	b♭	g
5	7	3		10	7	6
			Succession II	b♭	g	e
7	3	5		14	12	10
			Succession III	g	e	b♭
3	5	7		12	10	7

The results for both groups are given in Table I. Here "Pref. 3" refers to the preferences for ending the melody on ratio symbol 3, and "Pref. 5" for ending on ratio symbol 5. The results are expressed in percentages.

TABLE I

Percentage Preferences for Succession paired with

I {10, 7, 6 / e, b♭, g} II {14, 12, 10 / b♭, g, e}

Order presented		Pref. 3	Test	Pref. 5
1st. 10, 7, 6		60	A	40
2nd. 14, 12, 10		49	B	51
1st. 14, 12, 10		60	A	40
2nd. 10, 7, 6		53	B	47
	Averages	60	A	40
		51	B	49
		56	A and B	44

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 56 : 44.

Percentage Preferences for Succession paired with

I {10, 7, 6 / e, b♭, g} III {12, 10, 7 / g, e, b♭}

Order presented		Pref. 3	Test	Pref. 7
1st. 10, 7, 6		60	A	40
2nd. 12, 10, 7		76	B	24
1st. 12, 10, 7		73	A	27
2nd. 10, 7, 6		64	B	36
	Averages	67	A	33
		70	B	30
		68	A and B	32

Conclusion: when ratio symbol 3 is paired with 7, symbol 3 is preferred as an ending in the proportion 68 : 32.

Percentage Preferences for Succession

II {14, 12, 10 / b♭, g, e		paired with		III {12, 10, 7 / g, e, b♭
Order presented		Pref. 5	Test	Pref. 7
1st. 14, 12, 10		60	A	40
2nd. 12, 10, 7		64	B	36
1st. 12, 10, 7		46	A	54
2nd. 14, 12, 10		56	B	44
	Averages	53	A	47
		60	B	40
		56	A and B	44

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 56 : 44.

Total Percentage of Preferences for Successions I, II, and III

	Pref. 3 I	Pref. 5 II	Pref. 7 III
		Test A	
Men	42	30	28
Women	43	32	25
Total	42	31	27
		Test B	
Men	42	38	20
Women	38	35	27
Total	40	37	23
Average for both tests	41	34	25

Conclusion: the ratio symbols 3, 5, and 7 respectively, are preferred as endings in the proportions 41 : 34 : 25.

Test C

It so happened that g (3), the most preferred note, was also the lowest note. To eliminate this effect of falling inflection, a second combination was arranged. The triad was here sounded in the following order,—first the high note, then the low note, and last the middle note.

Ratio Symbol				Vib. Ratio in Time-Order Presented		
				1st	2nd	3rd
			Succession IV			
				b♭	e	g
7	5	3		14	10	12
			Succession V			
				g	b♭	e
3	7	5		12	7	10
			Succession VI			
				e	g	b♭
5	3	7		10	6	7

Thus where the succession g b♭ e was paired with e g b♭, g was the high and b♭ the low in the former case, and e was the high and g the low in the latter.

TABLE II

Percentage Preferences for Succession

IV {14, 10, 12 / bb, e, g}	paired with	V {12, 7, 10 / g, bb, e}
Order presented	Pref. 3	Pref. 5
1st. 14, 10, 12 / 2nd. 12, 7, 10	51	49
1st. 12, 7, 10 / 2nd. 14, 10, 12	56	44
Average	54	46

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 54 : 46.

Percentage Preferences for Succession

IV {14, 10, 12 / bb, e, g}	paired with	VI {10, 6, 7 / e, g, bb}
Order Presented	Pref. 3	Pref. 7
1st. 14, 10, 12 / 2nd. 10, 6, 7	62	38
1st. 10, 6, 7 / 2nd. 14, 10, 12	62	38
Average	62	38

Conclusion: when ratio symbol $3\frac{1}{2}$ is paired with 7, symbol 3 is preferred as an ending in the proportion 62 : 38.

Percentage Preferences for Succession

V {12, 7, 10 / g, bb, e}	paired with	VI {10, 6, 7 / e, g, bb}
Order presented	Pref. 5	Pref. 7
1st. 12, 7, 10 / 2nd. 10, 6, 7	57	43
1st. 10, 6, 7 / 2nd. 12, 7, 10	61	39
Average	59	41

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 59 : 41.

Total Percentage Preferences for Successions IV, V, and VI
Test C

	Pref. 3 IV	Pref. 5 V	Pref. 7 VI
Men	42	37	21
Women	35	33	32
Total	39	35	26

Conclusion: the ratio symbols 3, 5 and 7 respectively, are preferred as endings in the proportions 39 : 35 : 26.

Averages

Averages have been computed for all three groups, and are shown in Table III. Here are contrasted the possible melody-endings,—on vibration-ratios 6 or 12 (g, ratio symbol 3); on vibration-ratio 10 (e, ratio symbol 5); and on vibration-ratio 7 (b♭, ratio symbol 7).

TABLE III

Averages for Tests A, B, and C

Ending on 6 or 12 (3)		paired with Pref. 3	Ending on 10 (5) Pref. 5
1st. 3 / 2nd. 5		53	47
1st. 5 / 2nd. 3		57	43
	Average	55	45

Conclusion: when ratio symbol 3 is paired with 5, symbol 3 is preferred as an ending in the proportion 55 : 45.

Ending on 6 or 12 (3)		paired with Pref. 3	Ending on 7 (7) Pref. 7
1st. 3 / 2nd. 7		66	34
1st. 7 / 2nd. 3		66	34
	Average	66	34

Conclusion: when ratio symbol 3 is paired with 7, symbol 3 is preferred as an ending in the proportion 66 : 34.

Ending on 10 (5)		paired with Pref. 5	Ending on 7 (7) Pref. 7
1st. 5 / 2nd. 7		60	40
1st. 7 / 2nd. 5		54	46
	Average	57	43

Conclusion: when ratio symbol 5 is paired with 7, symbol 5 is preferred as an ending in the proportion 57 : 43.

Total Percentage Preferences for 3, 5, 7
Average for All Tests

	Pref. 3	Pref. 5	Pref. 7
Men	42	35	23
Women	39	33	28
Total	41	34	25

Conclusion: the ratio symbols 3, 5 and 7 are preferred as endings in the proportions 41 : 34 : 25.

Summary

(1) When ratio symbol 3 is paired with 5, the difference in the ending-preference is small; but 3 is favored, in the proportion 55 : 45.

(2) When ratio symbol 5 is paired with 7, the difference in the ending-preference is small; but 5 is favored, in the proportion 57 : 43.

(3) When ratio symbol 3 is paired with 7, the difference in the ending-preference is quite marked, and 3 is favored, in the proportion 66 : 34.

(4) When compared with ratio symbols 5 and 7, 3 apparently gives more finality than 5, and 5 more than 7, in the proportion: symbol 3, 41%; symbol 5, 34%; and symbol 7, 25%.

(5) Including the results of Meyer on symbol 2, and reducing his figures to data comparable with the results of these experiments, the proportions would be: symbol 2, 58%; symbol 3, 17%; symbol 5, 14%; and symbol 7, 11%.

(6) There seems to be little sex-difference in the ending-preferences.

THE CONTROLLED *VS.* THE FREE COMPLETION

By Rose S. Malmud, Teachers College, Columbia University

It is the purpose of the writer to present briefly for comparison the type of language completion standardized by Trabue (introduced by Ebbinghaus)[1] and the selective or controlled completion. The graded language completion scale is recognized as a measuring instrument. Mere recognition, however, does not establish its verity, and the questions persist: (1) what does the instrument measure? and (2) how perfect an instrument is it? To answer that it measures language ability is no explanation. For what is language ability, as measured by the completion, and what makes it an index of general intelligence? Clearly it is not mere word knowledge. Sentence completion deals with words not in their isolation but in their connectedness, their refined interrelation and juxtaposition. As Lafcadio Hearn has picturesquely said, "Words are very much like lizards; they change color according to position."

In this connection the classical terminology of association will be abandoned as unfruitful in favor of a terminology of habit formation. The unit of the sentence is interpreted not as the word but as the word-habit. Thus, a word in its relation to other words may be univalent, bivalent, trivalent, or multivalent according as it coordinates or integrates into a larger unit with one or two or more elements in a given sentence. A simple sentence represents a simple integration; a complex sentence, a hierarchy of minor integrations or word-habits. A boy who writes "heart" in the first blank of the completion "One's..... in life.....upon so.....factors that it is not..... to state any single.....for failure" illustrates the use of a univalent word. The integration was too complex for the boy, and he responded to the only word for which an habitual response was available. Or similarly, when another individual responds to the word "friend" by writing "old" in the third blank of the completion "..... that are ..... to one by an ..... friend should be pardoned ..... readily than injuries done by one ..... is not angry." On the other hand, two discrete integrations may take place; as when a child in response to the incomplete sentence "It is very annoying to .... toothache, ... often comes at the most ..... time imaginable," writes "It is very annoying to have a toothache, rain often comes at the most unexpected time imaginable." We may therefore say with some truth that

[1] M. R. Trabue, *Completion-Test Language Scales*, New York City, Teachers College, Columbia University, 1916; H. Ebbinghaus, *Z. f. Psych.*, 13, 1897, 401-459.

a sentence completion measures the extent to which certain hierarchies of word-habits have been formed.

The free completion has been called the creative completion. This description will bear critical examination. The writer, acting as *S* in L. Hunseker's study, at the Institute of Educational Research, Teachers College, Columbia University, of levels of difficulty as related to speed, had the opportunity of observing her procedure in completing sentences. Two types of difficulty were noted: (1) the difficulty that arose from a great number of omissions which called for complex integration; (2) the difficulty that was the result of lack of familiarity with the implied context of the sentence, *i. e.*, a situation for which no habitual integration was forthcoming. For example, two incomplete sentences may be cited, both of which had an equal number of omissions, but to which the responses were different. (1) "Ibsen in the social ..... in ..... ways has struck the highest ..... of modern dramatic ....." (2) "Throughout the river plains of northern India, two harvests, and ..... some provinces ..... are ..... each ....." To the first the response was that of immediate recognition. It was as if the blanks were not there, and the writer responded to it as a co-ordinated whole, writing the omitted words *in order*. To the second, however, the response was that of a feeling of strangeness, a complete lack of familiarity. The coordination had to be built up from discrete word-habits, a process which may be presented in the form of a verbal graph as follows, indicating the order in which the responses were made and discrete word-habits were integrated:

(1) Harvests are .....
(2) Harvests are ..... each .....
(3) Two harvests, and ..... some provinces, are ..... each year
(4) Two harvests, and ..... some provinces ..... are ..... each year.
(5) Throughout the river plains of northern India, two harvests, ... in some provinces ..... are ..... each .....

This response, or rather series of responses, had all the feeling tone of creative thought: that of unfamiliarity, persistence in spite of unhabitual conditions, attentive examination of all the elements, trial procedure from the known to the unknown, selection of multivalent words—habitual qualifications of time, place, number,—made with a view to possible integrations. Apparent success brought all the elation attending a fresh and individual discovery. Compare the time of 36.8 sec. taken by the first response with 243.8 sec. required by the second. This difference represents the time it took for the piecemeal building of discrete habits into a new integration. It is this type of com-

pletion which, in the opinion of the writer at least, is significant as evidence not so much of language ability as of general intelligence.

The question may well be raised as to the relation of integrations already formed to intelligence. Creative thought as here understood is itself a complicated habit or integration of persistent search, attentive examination, and trial solving. It is the direct measure of this habit therefore that should be of interest, and not its indirect and uncertain manifestations. It is conceivable that if a man who was a stoker because he could be nothing else were placed at one end of a scale, and Newton were placed at the other, with (say) the writer half-way between —it is conceivable that both the stoker and the writer could respond better to a battery of up-to-date information tests so prominent in intelligence measurement than could Newton. Our knowledges and verbal habits are not necessarily or frequently the result of feelings of inadequacy, attitudes of persistent searching, and trial integrations. All this has been short-circuited for us and neatly classified as pre-formed truths. To make our meaning more apparent,—one of Newton's earlier discoveries was the composition of white light. It appears, however, that he assumed that all prisms produced spectra of equal lengths. Any college freshman who had taken a course in physics could tell him something of the varying dispersiveness of varying refractive media. But it was Newton's type of ability that made other investigations, and verifications, and the freshman's erudition possible. Similarly, any modern student of medicine could cite anatomical truths which would be a revelation to Vesalius, but it was Vesalius' habits of original thought, his questionings of the revered oracular utterances of a Galen, which made those very truths an inevitable development. It is possible, further, for a modern farmer to add a cubit to the height of his fruit trees. But it was a Mendel who took thought. All this is by way of pointing out that it is not mere knowledges, ready habits, but the integration of these into unhabitual patterns and new productive combinations, the forging-ahead aspect of intelligence, which is of significance. In the free completion an original integration is sometimes actually penalized. In the I. E. R. completions, for example, children whose geographical training has not been adequately provided for will respond to "A body of ..... entirely surrounded by ..... is called an ....." by "A body of people entirely surrounded by people is called an mob," or "A body of man entirely surrounded by police is called an thief," or "A body of man entirely surrounded by earth is called an grave,"—and will receive no credit.

These conditions lead up to and in part answer our second question: how perfect a measuring instrument is the free completion? An obvious defect is the part that individual judgment in the scoring plays. What would be our reaction to a difference in the reading of a thermometer at a given time by doctors at a sick bed? Assuming that we know what is being measured, the personal equation determines the reading, and makes indefinite the status of the free completion as a measuring instrument.

The controlled completion, we believe, is an improved instrument in what it measures and in the accuracy with which it measures it. It is a better measure of language ability and a finer index of general intelligence. As a language scale it may be more accurately graded. Nuances of meaning may be tested to an extent not possible with the free completion. Thus in the completion "Many people ..... their health because ..... do not ..... the ..... of hygiene," equal credit according to the Trabue key is given to the responses "observe" and "understand." The assumption may be that temporary interference, emotional inhibitions, or what not, may prevent the child from responding with "observe," and that the two words are nearly enough synonymous to give them equal weight. In the controlled completion, however, temporary interference is neutralized, and the selection is equated. It is then possible to give more credit for the use of "observe," which is more discriminating and accurate. A more refined adjustment to the verbal situation is called for than is possible in the free completion. The habitual, unreflective, glib response is not always adequate as it is in the free completion. The controlled completion calls for perception of the difficulty, attentive examination of all the elements, selection of uni-, bi-, or trivalent words as the case may be for possible integrations, the result of which will be a coordinated whole or disjointed parts. The whole controlled completion represents a series of conflicting word-habits only one or two of which fulfill the conditions of the sentence. It does not depend for its scoring on subjective judgment. Inasmuch as it is not subject to the variation of individual opinion, it is more exact. Whatever errors are present are constant and inherent in its objectivity. Incidentally, where many individuals have to be tested, as in large schools or offices where time and labor saving devices are a desideratum, the controlled completion is to be commended. There is no need for complicated keys, subtle interpretations, or harassing wonder at what the individual meant or whether he meant anything at all.

The experimental data available are not as adequate as one would like. They are subject to errors of sampling, insufficient

cases for any one form, lack of equation of conditions at different schools, and inequality of criteria. The experimental procedure may be briefly described as follows.

The 8 forms that make up the Trabue Language Scale (forms B, C, D, E for the elementary grades, and J, K, L, M for high schools) were cast into the controlled form of completion (herewith appended). The number of words offered for selection in each case bears a constant relation to the number of words omitted and the words are arranged in alphabetical order. On the assumption that forms B, C, D and E are practically equivalent, each group tested was given one of the Trabue forms and an alternate form of the controlled completion (*e. g.*, Trabue Form B followed by Malmud-Trabue Form C or, with a succeeding group, Malmud-Trabue Form D followed by Trabue Form E, to neutralize any possible advantage of position). According to Trabue, the language ability of a child cannot be adequately measured with less than 3 of the forms (there are at most 10 graded completions in any one form). Our purpose, however, even if time permitted, was not primarily to measure the language ability of any one individual, but to compare two types of completions. One form of both types was therefore given in most cases. Whatever error this procedure introduces is present for both types of completion. The tests were tried between the months of February and June of 1924 in schools where a general intelligence or achievement test had been given which might be used as a criterion. The directions given for the Trabue Forms were: "Write one word on each inch of dots so as to make the whole sentence true and sensible"; for the Malmud-Trabue forms: "Write one word on each inch of dots so as to make the whole sentence true and sensible. Choose the word from those which are printed at the end of the sentence." One minute longer was allowed for each of the Malmud-Trabue forms than for the Trabue (*i. e.*, 8 min. for B, C, D or E, and 6 for J[2], K, L or M).

The results are tabulated on page 406.

Where feasible, schools which had been given identical intelligence or achievement tests within the same period of time were grouped together. However, Public School 42 and Public Schools 28 and 189, all of which had been given an intelligence test by the Institute of Educational Research, showed very little overlapping in the intelligence scores, although there was considerable overlapping of language completion scores. It was therefore deemed advisable to separate the schools as representing different types of groups. In Public Schools 28 and 189, representing fairly similar groups, three criteria were available: the National Intelligence Test, the Institute of Educational Research Intelligence Test and the Stanford Achievement Test. The groups within the schools taking these tests, however, were not identical: the groups which were given the I. E. R. Tests represent only 6B and 7A classes; the groups which were given the National Intelligence Test and the Stanford Achievement Test represent classes from the 4A to the 8A; classes which overlapped but were not identical. At Public

[2]Because of an error, only 6 of the sentences instead of the 7 in Form J of the Malmud-Trabue completions were made available.

School	Trabue Completion Form	n[1]	Pearson r[2] PE[3]	Criterion	Malmud-Trabue Completon Form	n[1]	Pearson r[2] PE[3]
P.S. 28 Manhattan	B	85	.48±.06	National Intelligence Test 1923	B	76	.34±.07
P.S. 189 "	C	78	.53±.05		C	109	.35±.06
	D	50	.59±.06		D	130	.54±.04
	E	134	.45±.05		E	50	.57±.06
P.S. 42 Bronx	B	91	.55±.05	National 1924	B	186	.63±.03
P.S. 144 Brooklyn	C	198	.58±.03		C	96	.55±.05
	D	105	.46±.05		D	141	.44±.05
	E	87	.41±.06		E	133	.60±.04
P.S. 28 Manhattan	B	49	.90±.02	Institute of Educational Research Tests of Intelligence 1923 " "	B	46	.38±.09
P.S. 189 "	C	47	.40±.08		C	51	.51±.07
	D	49	.60±.06		D	106	.63±.04
	E	108	.58±.04		E	49	.48±.07
P.S. 42 Bronx	B	48	.62±.06		B	58	.71±.04
	C	67	.55±.06		C	52	.56±.06
	D	72	.29±.07		D	77	.55±.05
	E	74	.56±.05		E	69	.44±.07
P.S. 28 Manhattan	B	80	.49±.06	Stanford Achievement Tests 1923	B	77	.35±.07
P.S. 189 "	C	79	.47±.06		C	83	.34±.07
	D	50	.50±.07		D	126	.51±.04
	E	131	.39±.05		E	50	.56±.07
Junior High School	J	82	(r Unlike- .43±.06	Otis Index of Brightness	J	72	(r Unlike- .48±.06
P.S. 178 Brooklyn	K	72	Signed Prs.) .43±.06		K	82	Signed Pairs) .34±.07
	L	85	.56±.05		L	100	.48±.05
	M	100	.61±.04		M	85	.73±.03
Speyer Junior High School	L, M	92	Pearson r .47±.05	National 1923	J, K	92	Pearson r .43±.06
Graduate Group in Teachers College Columbia Univ.	J, K	19	" " .65±.09	Thorndike Intelligence Test for High School Graduates (Forms IE, IIE, IIIAAA)	L, M	19	" " .67±.09

[1] = Number of cases
[2] = Correlation with the criterion
[3] = Probable error according to the PE_r table of H. A. Toops and Z. F. Miner, *Journal Educ. Research*, Jan., 1924

School 178, Brooklyn (Junior High School), raw scores made on the Otis Intelligence Test were available, but unfortunately for our purposes some pupils had been given the Otis Test in 1923 and others in 1924. However, an index of brightness had been determined for each pupil. Since the index of brightness was found to correlate .84 with the raw score, the correlation was thought good enough to permit the substitution of the index of brightness for the raw score. To ascertain approximate correlation in this case, therefore, the method of unlike signed pairs was used. The graduate group mentioned in the Table represented a class in mental measurements which had been given the Thorndike Intelligence Test for high school graduates, a fact which the writer was given the privilege of capitalizing for correlation purposes.

This investigation was made possible through the kindness of Professor E. L. Thorndike, who, as head of the Institute of Educational Research, provided the testing material.

Form B

1. We like good boys ..... girls.	along and are ask for said the
2. The ..... is barking at the cat.	angry bear cat child dog good hen
3. The stars and the ..... will shine to-night.	clouds dipper moon Orion shoes sky sun
4. Time ..... often more valuable ..... money.	also are because for is lost than though pocket with
5. The poor baby ..... as if it were ..... sick.	being coos colic is laughs looks pale teething very was
6. She ..... if she will.	came can does goes has is might
7. Brothers and sisters ..... always ..... to help .....other and should ..... quarrel.	along also always around angrily but each have kind loudly never out should some sometimes try
8. ..... weather usually ..... a good effect ..... one's spirits.	bad brews but gives has if in is not on pleasant poor then to vane
9. It is very annoying to ....toothache ..... often comes at the most.....time imaginable	a and bad best boast but dentist enjoy happy have inconvenient lucky pains therefore which worst
10. To ..... friends is always .... the ..... it takes.	best betray blame equal false given gifts good longer make our these treachery trouble worth

Form C

1. The sky ..... blue.	are dark deep is light rocket very
2. Men ..... older than boys.	all are be is not see women

3. Good boys.....kind..... their sisters.	and are by for girls men not of to words
4. The girl fell and ..... her head.	against cried hurt on over stumbled to
5. The.....rises.....the morning and ..... at night.	also always bird bread earliest every from in man sets shines sometimes sun therefore with
6. The boy who ..... hard ..... do well.	chews candy did enough example made never to will works
7. Men.....more..... to do heavy work ..... women.	are brains complain do except for have lack like made money strength than thinking without
8. The sun is so ..... that one cannot .... directly ..... causing great discomfort to the eyes.	and anything at big bright distant high it look low measure see setting therefore without
9. The knowledge of .... use fire is ..... of ..... important things known by ..... but unknown ..... animals.	all books careful cooking fireman for from how in man often one some the them to when who
10. One ought to ..... great care to ..... the right ..... of ..... for one who ..... bad habits ..... it ..... to get away from them.	acquires bad dislikes easy exercise finds form habits hard impression kind never often oneself pays people shows side smoking thing wrong

Form D

1. We are going ..... school.	at big evening high in public to
2. I ..... to school each day.	early go going have hate late like
3. The ..... plays ..... her dolls all day.	also ball child for house only tag to were with
4. The rude child does not ..... many friends.	against apologize care displease has make need
5. Hard.....makes.....tired.	break candy lazy one sick teeth thing times very work
6. It is good to hear ..... voice .... friend.	a and deaf dumb false hoarse loud nasal of poor singing speaking the to with
7. The happiest and.....contented man is the one ..... lives a busy and useful	also as bee least life most not pleasure poor things time too unhappy who whose
8. The best advice ..... usually ..... obtained ..... one's parents.	against always be been can children except follows from is not therefore to with works

9. .....things are.....satisfying to any ordinary ..... than congenial friends.	all conditions few friend hardly horrid man more not often one quite the these unpleasant
10. ..... a rule one ..... association ..... friends.	against although and as avoids dislikes enjoys in making not observing refuses scorns therefore with

Form E

1. I see you. Can you see .....?	eyes I look me not no yes
2. Ice is cold, but fire is	big bonfire burns cold hot out water
3. The kind lady ..... the poor man a dollar.	advised flings friend gave helps refused said
4. The best ..... to sleep is at night.	boy early go thing time way when
5. Children should ..... many lessons from ..... parents.	cruel do learn never obey other recite show their your
6. The child .... the river ..... was drowned.	and at deep fell flowed goes into looked playing said saw so swiftly was who
7. It is a ..... task to be kind to every beggar .... for money.	always and asks blind childish daily easy except extorts hard not poor refuses who works
8. Worry ..... never improved a situation, but has ..... made conditions	bearable better can excellent good hardly has more must never not usually which will worse
9. When one feels drowsy and ..... it ..... happens that he is ..... to fix his attention very successfully ..... anything.	able active alert always do energetic form has invariably must not on or seldom so tired
10. In order .... clearly at ....., it is ..... to ..... artificial	always coloring flowers foolish last light look manufacture night necessary one see something that think to use unnecessary

Form J

1. Boys and ..... soon become .....and women.	big boys children everybody girls grown-up men old poor rich
2. The ..... are often more contented ..... the rich.	also because boys except girls men parents poor than to
3. The rose is a favorite ..... because of ..... fragrance and	also and beauty flower height its kindness not pet scent smallness smell their thorns tree

4. It is very ..... to become ..... acquainted ..... persons who ..... timid.
are bad because except foolish friends hard if know like not speak unwise well with wrong

5. Extremely old.....sometimes ..... almost as ..... care as .....
and attention because bestow exercise healthy infants little much need negligible people rags read say sleep

6. One's ..... in life ..... upon so ..... factors ..... it is not ..... to state any single ..... for ..... failure.
because cause death depends easy exception expresses few hard health heart man many money much one's shows success that which woman

7. The future ..... of the stars and the facts of ..... history are ..... now once for all, ... I like them ..... not.
although and as astrology astronomy but course facts fixed ignored my or their unknown whether

Form K

1. The boy will ..... his hand if ..... plays with fire.
cut boys burn ever girls he holds likes use wet

2. Hot weather comes in the ..... and ..... weather ..... the winter.
basement cold comes hot in makes south summer the then this through tropics warm winter

3. The poor little ..... has ..... nothing to ..... ; he is hungry.
and child cry dish do done dragon eat glutton had never play said wanted wear

4. Very few people ..... how to spend time and ..... to the best advantage.
also can get know money spend try use want work

5. One ..... not, as a ....., ... attention ..... uninteresting things.
among can distract does from general give has keep not patience rule thing thinks to usual

6. To eat ..... one is ..... is a ..... pleasure.
and candy dinner full food great hungry nauseous not sad sick something what when with

7. ..... they ..... us..... not, nature's ..... are ..... and unchangeable.
and animals arts cruelty even finished fixed give laws or please since struggles that whether

Form L

1. Children ..... are rude..... not easily win friends.
also and but do perhaps sometimes then therefore to who

2. Plenty ..... exercise and ..... air ..... healthy ..... and girls.
bad boys children except food for fresh give makes need of sleep to wealthy wise with

3. In ..... to maintain ..... health, one should have nourishing .....
also always candy case clothing exercise food one's order poor sleep stead time vain warm

4. ..... happiness cannot be ..... with money.
borrowed bought contented good if no seen therefore true unless

5. One's ..... do ..... always express his thoughts.
also clothes good ideas not opinions reading sometimes therefore words

6. To ..... to wait, after having ..... to go ....., ... very annoying.
along bad have is it makes nowhere out patiently prepared refused said when wish with work

7. It is sometimes ..... to ..... between ..... of action.
but children choose difficult do done go guns kinds said see sit speeches therefore win

8. One can ..... do his ..... at one ..... while ..... of another.
also always away best chores dishes doing easily hair not place quarreling seeing sometimes task thinking

Form M

1. One cannot foretell ..... will happen in the ..... .
anything future history just past that time today what when

2. The dog ..... a useful ..... because ..... his intelligence and faithfulness.
although and animal barks does guards has is manner of tree trick watches when wood

3. Many people .....their health because ..... do not ..... the ..... of hygiene.
amount best children disease guard have injure if observe protect rules see they things understand when

4. Nothing can ..... one's happiness ..... effectively than a guilty ..... .
as conscience convict criminal destroy enjoy less make man more one so sometimes tell win

5. To ..... many things ..... ever finishing any of them ..... a ..... habit.
also and bad begin complete fine good is splendid spoils their unless was were when without

6. The ..... seems ..... and dreary ..... a discouraged ..... .
because bright cheerful dark discontent except happy in look man manner thing to voice with world

7. ..... that are ..... to one by an ..... friend should be pardoned ..... readily than injuries done by one ..... is not angry.
and angry bestowed but done faithless favors generous injuries less more therefore unknown very who

8. It is ..... that a full-grown man should ..... a ghost ..... he is ..... .
and able become believe but destroy improbable pale said see sober true unless welcome when wrong

VISUAL PERCEPTION OF MOVEMENT

By Forrest L. Dimmick and Howard G. Scahill,
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In a previous investigation,[1] the perception of movement from two stationary stimuli and the phi-phenomenon proposed by Wertheimer as the extra-sensory basis of this perception have been critically examined; the compulsory conditions for their arousal have been established; and the reflection of these compulsory conditions in consciousness has been shown to "take the form of a primary integration of a visual quality (grey) with a duration which is characteristically brief." This grey flash is independent of the quality of the stimulus, and is directly determined by the temporal interval between the exposures of the two stimulus-members. In the present investigation we undertook to determine the differences between the perception set up by the presentation successively of two stationary stimuli and that set up by the presentation of an actually moving stimulus.

Procedure I

Our apparatus was a form of the Dodge tachistoscope similar to that used by Dimmick, to which we added a further modification to present an actually moving stimulus, which in every way duplicated the fields, positions, and time-intervals of the successive stationary stimulus.

This moving field was actuated as follows. A small wire cable was fastened to the top and bottom of the falling shutter and ran over pulleys above and below, so that the pulleys turned when the shutter fell. One end of a second cable was wound three times around the shaft of the lower pulley. The other end ran through the wall into the dark room. In the dark room was our fourth field, a movable background attached above and below to springs. The lower spring was fastened to the table upon which the apparatus rested, and the upper spring was fastened to the second wire cable. The ratio of the lower pulley to its shaft was such that, while the shutter was falling through its distance which gave an exposure interval of 90 σ, the second wire cable moved the same distance as that between the two members of the stationary stimulus (1 in.). The resistance of the springs was adjusted so that the movable background remained stationary at the beginning and end of the exposure for the same length of time that the members of the stationary stimulus were exposed (30 σ). Above the moving field was a light box which admitted light to the field when a shutter in the bottom was opened. This shutter was controlled by a solenoid with a contact on the falling shutter so placed that the light shutter was open for a time equal to the total exposure-time of the stationary stimulus.

[1] This Journal, 31, 1920, 317 ff.

The stimuli were (1) two stationary black horizontal lines (1 by 1/4 in.) on a white background (the one appeared for 30σ, and after an interval of 90σ the other appeared an inch below for 30σ); (2) an actually moving black line identical with those of (1), which appeared stationary for 30σ, moved down one inch during a 90σ interval, and then remained stationary for 30σ, after which it disappeared.

In our first procedure we used Dimmick's "process instructions:" "A stimulus will be shown you which will arouse a visual perception. Describe this perception in strictly psychological terms as accurately as you can. Report no process of which you are not sure."[2] The instructions were given to every *O* at the beginning of every observation hour. The four *O*s were: W. P. Williamson (W), beginning student in psychology; F. L. Dimmick (D); T. C. Schneirla (S), assistant in psychology; and A. C. Anderson (A), assistant in psychology. Only one *O*, D, knew the problem of the experiment.

O sat in the dark room for 15 min. before beginning his observation. The proper auxiliary shutters were opened, a ready signal was given, and the falling shutter was tripped. *O* gave his report to *E* through a speaking tube. The two stimuli were given an equal number of times in a haphazard order. On coming from the dark room *O* was asked to make drawings of his experiences.

Results

All *O*s gave reports of movements in practically all cases (90%) both with the stationary stimulus and with the moving stimulus. Not only did they experience movement with both stimuli, but the similarity of description was very marked. No *O* noted that there were two kinds of stimuli. The following reports are representative.

Stationary Stimulus

A "A vague bar appeared and disappeared. Then I saw two bars, one near where the first had been and one an inch below and a shadow moving between them. I saw the bars simultaneously but I could not get the time-relation of the movement." (See Fig. 5.)

W "A bar appeared distinctly above the center of the field and moved down. It was a blur all the way down. It apparently stops before the light goes out, for it gets more distinct at the bottom."

S "An object appeared above the center and soon changed position to a point below. The upper position was fainter than the lower one. A blur appeared between." (See Fig. 3.)

D "Very quick, flashy movement. The line was definite in the first position; flashed downward and disappeared; that is, there was a sharp, definite black line, with a grey patch below it and attached to it." (Similar to Fig. 4.)

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[2] *Ibid.*, 319 f. The "meaning" instructions were also tried; but owing to their lack of systematic training, the *O*s did not appreciate the point of difference between the two instructions, and gave both "process" and "meaning" reports under each.

Moving Stimulus

A "I saw a clear cut bar, then a sort of blur which extended down from that bar about an inch; then a second bar appeared at the bottom." (See Fig. 8.)

W "Above the center the bar appeared quite distinct, clear-cut and black, and moved downward to below the center. The movement was a blur about the same color as the background except that it was a bit darker."

S "The object appeared near the top rather definite, moved rapidly down, and disappeared in this movement; that is, its movement was a grey streak and grew fainter at the bottom, and went out into nothing. The object did not appear below." (See Fig. 9.)

D "A very good movement—the line was definite in the upper and lower positions. None of the intermediate positions was definite. Just below the top black line was a space, and below this was a patch of grey attached to the lower position. The patch was streaked horizontally light and dark grey." (Similar to Fig. 10.)

Procedure IIa

Because the *O*s had made no spontaneous distinction between the two stimuli presented, we altered the instructions to determine, if possible, whether they *could* distinguish them; and, if so, what criteria they used. The procedure was the same as before, with these instructions: "You will be presented two different stimuli; one of the stimuli is moving, the other is not. You are to report first whether the stimulus presented is the moving or the non-moving stimulus; and secondly, you are to report the basis of your judgment."

Results IIa

	Trials	No Perception of Movement	Perception of Movement	?	Reports of Non-Mov. Stimulus	Reports of Moving Stimulus
O		W D A S	W D A S	W D A S	W D A S	W D A S
Stationary Stimulus	1st 10	2 1 1 4	8 9 7 6	0 0 2 0	0 7 5 0	0 2 0 0
	2nd 10	1 0 4 2	9 10 5 6	0 0 1 2	0 6 3 0	0 4 1 1
	3rd 10	2 0 4 2	8 10 6 5	0 0 0 3	0 10 6 0	0 0 1 0
	4th 10	0 – 2 –	10 – 8 –	0 – 0 –	0 – 7 –	0 – 0 –
	5th 10	0 – 3 –	10 – 7 –	0 – 0 –	0 – 7 –	0 – 0 –
Moving Stimulus	1st 10	0 0 0 5	10 10 10 5	0 0 0 0	0 3 0 0	0 7 9 1
	2nd 10	1 0 2 4	9 10 6 6	0 0 2 0	0 4 2 0	0 6 6 0
	3rd 10	0 0 2 5	10 10 7 3	0 0 1 2	0 0 1 0	0 10 7 0
	4th 10	0 0 2 –	10 9 7 –	0 1 1 –	0 1 1 –	0 8 7 –

The Table shows quantitatively the results of this procedure. With the non-moving stimulus all *O*s called the experience "movement" in 50-90% of the cases. The percentages for the moving stimulus were similar. This means that "moving" and "not moving" did not express characteristic differences between the two perceptions. Two of the *O*s (D and A) carried

out the instructions and made the distinction between the two stimuli in 70% of the cases. The other two *Os* (W and S) did not report the distinction, but gave descriptive differentiations in their reports. The outstanding fact is that, even though the *Os* were told that one stimulus was moving and that the other was non-moving, and were required to distinguish between them, they reported movement for both.

The following quotations from the *Os*' reports indicate the difference in the perceptions with the two stimuli.

With the *moving* stimulus the *Os* described their experiences as follows: W "equally intense throughout the movement," "not as fast"; D "movement more deliberate," "continuous," "could see it all the time in the movement," "slow movement," "steady movement," "saw intermediate positions," "perceived line as it moved"; A "no break in continuity," "did not lose track of it at any time," "could see it all the time," "no break," "no flicker, it was continuous," "I can keep stimulus in sight while it changes position"; S "moves slower for streak was rather dark," "not jerky but smooth," "could see object as it moved," "saw it at all points."

For the *non-moving* stimulus the *Os* report: A "a discontinuity of movement"; D "the speed of this movement is greater," "by speed I do not mean a temporal element, but a pattern of qualitative and spatial elements," "there is a clear-cutness of the spatial end-positions and rapid movement between them," "a smooth even patch of grey instead of a mottled one"; W "not smooth," "irregular," "jumped," "fast movement," "hesitation in movement," "rapid," "uneven," "jerky," "unsteady"; S "the object moved rapidly down," "the streak left behind the object was not as clear as before (when the stimulus was moving)," "I saw a streak between the positions; the movement was rapid because the streak was vague."

The *Os* perceived movement as frequently from the non-moving stimulus as from the moving stimulus. They all set up criteria and made distinctions between the stimuli, though in many cases they did not know that they were using them as criteria and did not report that they made the distinction. All of the perceptual experiences were primarily experiences of movement.

Procedure IIb

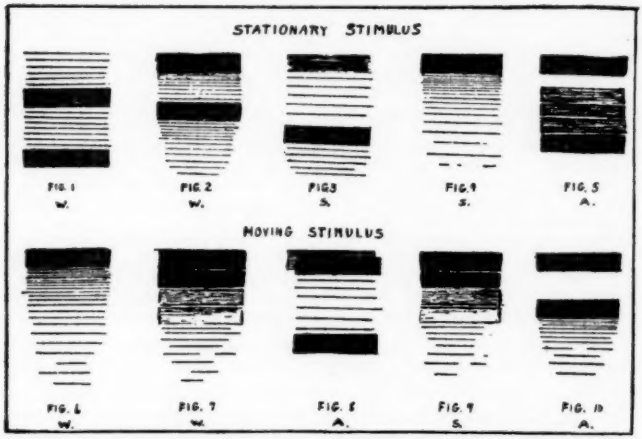
Inasmuch as none of the *Os* formally and consciously distinguished between the two stimuli, we changed the instructions to set up the stimulus attitude ('stimulus error') and thus to emphasize the distinguishing of the two experiences. The new instructions were: "You will be presented with two different stimuli, both of which will appear to move. Try to distinguish between them and give the basis for your distinction." Only two *Os* were available for this procedure.

Results

W was given 30 trials with each stimulus before he began verbally to distinguish the two stimuli. Then for 33 trials he shifted from one criterion to another until with the 64th trial he settled upon "even" and "uneven" as the proper distinguish-

ing characterizations. Of the 89 remaining presentations he was wrong in 4 cases, and there were two others in which he was not sure. This left him correct in 93% of the cases after he had developed the criterion of "evenness." "Even movement" was his characterization of the perception from the moving stimulus.

He described the two perceptions as follows. "In uneven movement, I see places in which the bar is more intense than in others. This leads me to believe that the bar is going slower in some places than in others." Further characterizations were: "it varies in intensity"; "the bar appears more as two flashes than as one continuous movement; at the indistinct



FIGG. 1-10 are copies of drawings made by 3 *O*s to supplement their verbal descriptions. Figg. 1-5 represent what the *O*s saw when the "stationary stimulus" was presented; Figg. 6-10 show the appearance of the "moving stimulus". Their respective verbal descriptions are given in the samples of reports.

points there is just a grey blur against the background"; "it appears as two separate motions—as it moves down it appears more intense at a point half way down,—it fades out, then jumps for a space and gets more intense, yet is still hazy"; "the bar moves down and disappears half-way below center—it seemed to come out of the background and become more distinct until it was black in the middle of the movement; then it faded and then became black again. The two places at center and end of the movement were deep black and clear-cut, while the rest was an indistinct grey of different intensities" (see Fig. 1); "the bar changes in intensity throughout the movement in 4 phases, intense, less intense, intense, less intense" (see Fig. 2). Even movement "keeps the same intensity throughout"; there is "no vibration"; "it is slower movement; its intensity falls off slowly at the end of the movement" (see Fig. 6); "a bar appears in the center, clean-cut and black, and moves downward, keeping this distinctness for about 1/3 of the movement, *i. e.*, it moves by steps; from there on it

decreases in intensity. By steps I mean I could *see* the bar in several positions and they were all seen at once, for at least some few steps" (see Fig. 7).

D was given 31 presentations under this procedure. After the first 6 he made the distinction. Of the remaining 25, 5 were wrong and of 3 he was uncertain. Thus he was correct in 68% of the cases. He used as his criteria "rate of movement" and "distinctness of positions." After his last observation he reported "the rate of movement is not adequate. The pattern of greys in the movement field is more distinctive. Whenever it was possible for me to get the pattern of one or the other distinctly I knew I was right, that is, there were some extreme and distinct patterns but many were intermediate. My other criteria were position in the field, size of the stimulus, outline of the stimulus, color or intensity, the absence of one member. All these factors came in but varied with the two main factors, often obscuring them and rendering my report uncertain." Thus the two stimuli can be distinguished, but only after much practice,—and then the distinction is not that of two different perceptions but only of two modifications of the same perception. The differences were more marked in the process-pattern than in the perception as such.

Conclusions

1. The present work confirms the previous work which brought out the process-pattern of movement in the form of a "filmy grey flash."

2. A moving stimulus does not give a perception of movement different in kind from that set up by a non-moving stimulus. The two perceptions are distinguishable not as experiences one of which moves while the other remains stationary, but as two forms of movement, *e. g.*, fast and slow. The corresponding process-patterns differ not in their essential form but merely in the relative distributions of the various degrees of intensity within the patch of grey, *e. g.*, greater or less uniformity. Physical movement of the stimulus, then, is not an element of the compulsory conditions of the perception of movement.

POPULAR ANSWERS TO SOME PSYCHOLOGICAL QUESTIONS

By H. K. Nixon, Columbia University

1. *Introduction*

The keenest class-room discussions in psychology center around such questions as telepathy, pre-natal influence, external signs of character, and the like. The beginning student especially, while willing to admit ignorance on many technical questions, comes to psychology with certain concepts of causation in human behavior quite firmly fixed. One of the common duties of the instructor is to help him sort over this initial stock of ideas, discarding some and working others into a more scientific formulation. A survey, therefore, of the prevalence of such common ideas may be of decided value to the psychologist as a starting point for his educational programme. The results of such a survey are presented in this article.

This field has been investigated in a more general way by several writers. Conklin (*1*) presents material on supersititious belief and practice among college students. He found that 82% recalled that they had believed superstitions at one time or another, 73% of the men thus reporting and 90% of the women. 40% of the males and 66% of the females admitted present superstitious belief or practice. Conklin thinks that the superstitions of women are more concerned with home, company, social relations, wishes, love, marriage and death, those of men with sports and success in business. In a later report (*2*) he cites the results of a very early investigation (*3*) in the same field to bear out his conclusions. Gould (*4*), Peters (*5*), and Dresslar (*6* and *7*) have likewise made special studies of this subject.

The purpose of the present investigation was somewhat more specific than that of those noted above, in that the primary interest was not in such general superstitions as those connected with black cats, spilled salt, and the like, but in beliefs which have greater scientific sanction. For that purpose, the list of 30 shown in the following questionary was selected, and presented in the form shown.

DIRECTIONS

Below are a number of statements. Some of them you may consider true, some of them false. If you think a statement is substantially true, draw a circle around T; if the statement is false, then a circle around F. Where it seems debatable, mark as seems to you nearest right.

1. T F The number of man's senses is five.
2. T F A child comes into the world with an instinctive knowledge of good and evil. This is his conscience and is born in him.
3. T F Certain lines in a person's hand foretell his future.
4. T F If you will stare at a person's back you can make him turn around. This is a form of telepathy.
5. T F It really is unlucky to have anything to do with the number thirteen.
6. T F A man's character can be read by noting the size and location of special developments of his head.
7. T F People with greenish eyes are not as trustworthy as people with blue or black eyes.
8. T F An expectant mother by fixing her mind on a subject can influence the character of her unborn child.
9. T F Women are inferior to men in intelligence.
10. T F People born under the influence of certain planets show the influence in their characters.
11. T F Intelligence can be increased by training.
12. T F Long, slender hands indicate an artistic nature.
13. T F Beginning an undertaking on Friday is almost certain to bring bad luck.
14. T F If a man but had faith enough he could heal a broken limb instantly.
15. T F Many eminent men have been feeble-minded as children.
16. T F Some animals are as intelligent as the average human.
17. T F No defect of body or mind can hold us back if we have enough will power.
18. T F Adults sometimes become feeble-minded from overstudy.
19. T F All men are created equal in capacity for achievement.
20. T F The marriage of cousins is practically certain to result in children of inferior intelligence.
21. T F Especially intelligent children are likely to be weak and retarded physically.
22. T F The study of mathematics is valuable because it gives one a logical mind.
23. T F A square jaw is a sign of will power.
24. T F You can estimate an individual's intelligence pretty closely by just looking at his face.
25. T F A high forehead indicates intellectual superiority.
26. T F Fear is unnatural. It is a bad habit.
27. T F Women are by nature purer and better than men.
28. T F A person who does not look you in the eye is likely to be dishonest.
29. T F Man is superior because his conduct is very largely guided by reason.
30. T F Any physical or mental disease can be contracted by thinking about it.

NAME..... Sex.....

It will be observed that the ideas here expressed range from some which are quite apparently pure superstitions to some that would occasion considerable discussion among a group of

scientific men. It was the intention to make use of ideas which in the main have had the stamp of disapproval placed upon them by modern investigations. It was, however, found impossible to phrase the statements in such a manner as to avoid all chance for quibbling and charges of ambiguity. In general it seems likely that if the list were submitted to a group of psychologists there would be less than 1% of unqualified affirmative answers. The difference between that small percent. and the averages for the groups which were secured to take part in this investigation may serve as a rough indicator of the present prevalence of unscientific beliefs among the educated members of our population.

The subjects of this investigation were 140 women and 219 men, students in elementary psychology in Columbia University, Columbia University Extension, and evening classes in New York University. About 85% came from the two latter institutions. Students in these groups are on the average somewhat more mature than the ordinary college student; and it is probable that the sample afforded is more nearly representative of the intellectual and cultural level of people of average and above the average level than would have been the case with day-students in colleges and universities.

The questionary was presented to these students without any comment or discussion other than the reading of the directions. They were allowed to take their own time in making the answers. In a few cases there were questions, in which instances the student was instructed to answer as seemed to him nearer right. In general the time taken was about 8 min. The questions usually served to arouse much discussion and demands to be told the answers and to be informed as to scores.

2. The Results of the Questionary

A. Number of the 30 Statements marked as True. We have already noted that presumably not more than one or two of the statements of the questionary would be marked as true by a group of trained psychologists. It is interesting to compare that estimate with the actual performance of the group under consideration. Figures are presented in Table I.

The figures of Table I need little explanation. It will be noted that the males on the average marked between 10 and 11 statements as true, while the women average slightly more than 12. Considering the S. D.'s, these differences are probably significant. The distribution for the men shows a slight tendency to bimodality, probably due to the inclusion of 25 Columbia College men who showed an inclination to mark relatively few of the statements true. The average number marked by this group was 8.08. Along with this may be noted the tendency

TABLE I

Number of the 30 Statements in the Questionary Marked as True

Number of Ss marking	Statements as True	Males	Females
" " " "	1 " " "	5	0
" " " "	2 " " "	4	1
" " " "	3 " " "	3	1
" " " "	4 " " "	10	3
" " " "	5 " " "	12	3
" " " "	6 " " "	11	7
" " " "	7 " " "	17	2
" " " "	8 " " "	18	9
" " " "	9 " " "	11	10
" " " "	10 " " "	14	11
" " " "	11 " " "	14	9
" " " "	12 " " "	14	8
" " " "	13 " " "	18	12
" " " "	14 " " "	16	18
" " " "	15 " " "	11	9
" " " "	16 " " "	4	8
" " " "	17 " " "	10	10
" " " "	18 " " "	9	4
" " " "	19 " " "	4	7
" " " "	20 " " "	9	6
" " " "	21 " " "	1	0
" " " "	22 " " "	0	0
" " " "	23 " " "	0	1
	Total	219	140
	Av. =	10.5	12.3
	S. D. Dis. =	5.0	4.4
	S. D. Av. =	3.3	.37

for the males as a group to be somewhat more variable than the females. Considering the groups together, it is to be seen that on the average a little more than one-third of the statements are marked true. Persons marking only 2 as true and persons marking more than 20 will be exceptional. We cannot be sure what it means to mark only a few. It may mean greater familiarity with scientific knowledge, and it may merely mean greater caution. On the other hand, a person who marks 20 or more of the statements true is probably relatively ignorant of the point of view of modern psychology. While no correlations of class-grades or intelligence-scores with the number marked as true on this test have been made, it is interesting to note that some students who marked 20 or more have done very creditable class work.

B. Percent. of Ss Believing each Statement. The question of the average number of statements marked true may be considered from another standpoint, that of the proportion of students believing any particular statement. This question may be studied from Table II.

TABLE II

Relative Prevalence of the 30 Beliefs

Statement No.	Idea involved	% of *Ss* marking as True		Order of prevalence	
		Males	Females	Males	Females
11	Intelligence increased by training	79%	80%	1	2
22	Math. gives logical mind	76	81	2.5	1
1	Number of man's senses five	76	78	2.5	3
29	Man superior because of reason	64	58	4	5
18	Adults feeble-minded from study	56	57	5	6
24	Face shows intelligence	50	62	6	4
12	Artistic nature indicated by fingers	42	52	7	9.5
6	Character indicated by head-shape	40	51	8	11
17	Will power supreme	39	52	9.5	9.5
25	High forehead shows intelligence	39	42	9.5	19
8	Prenatal influence	38	44	11.5	17.5
27	Women purer by nature	38	53	11.5	8
15	Early feeble-mindedness of great men	36	25	13.5	22.5
4	Telepathic influence of staring	36	56	13.5	7
26	Fear unnatural. A bad habit	35	47	15	15.5
23	Square jaw a sign of will power	34	50	16.5	12.5
20	Marriage of cousins gives deficients	34	50	16.5	12.5
28	Shifty eye shows dishonesty	33	47	18	15.5
21	Bright children physically retarded	32	33	19	21
2	Instinctive knowledge of good	31	39	20.5	20
30	Physical disease by thinking of it	31	44	20.5	17.5
16	Some animals superior in intelligence	25	48	22	14
19	Men created equal in capacity	23	20	23	24.5
9	Women inferior to men in intelligence	16	5.7	24	26.5
10	Character influenced by planets	15	20	25	24.5
14	Broken limb healed by faith	9	5.7	26	26.5
3	Lines in hand foretell future	8	25	27	22.5
7	Green-eyed people untrustworthy	4	4	28	28
5	Thirteen unlucky	1	0	29	30
13	Friday unlucky	1-	.7	30	29

It will be seen from this Table that about 80% of the *Ss* thought that intelligence can be increased by training, this being held true to about the same extent by males and by females. More males believed this statement than believed any other, and it was second in popularity with the females. On the other hand, the ideas connected with thirteen and Friday are relatively unpopular, at least as measured by this method. On the basis of Conklin's work it is probable that many of the *Ss* are actually influenced in their actions by these popular superstitions, but most refuse to mark them as true.

There appears a general tendency for the misconceptions most widely held—those at the top of the Table—to be due to ignorance or disagreement with the specific sense in which the psychologist uses the terms involved, as *e. g.* intelligence. The difficulty is then a simple one of lack of information. In the middle of the list fall ideas which are widely held, which are very popular, and which are often exploited by various agencies, such as the newspapers, but which have been definitely dis-

credited by the man of science. It is here that class discussions wax the hottest, and it is on these subjects that the instructor in elementary psychology should fortify himself with as much concrete evidence and as many experimental data as possible. At the lower end of the list appear popular superstitions which are widely known and acted upon, but which are usually quite frankly recognized for what they are.

The two columns at the right in Table II show the order of prevalence of the ideas with men and with women. In general the two orders are seen not to differ in any very marked manner. The greatest difference is in the case of No. 25, the idea that a high forehead indicates intelligence, which is considerably more popular with the males than with the females. Males are also somewhat stronger in the belief that many eminent men have been feeble-minded in childhood, and that the character of the unborn child may be influenced by the thoughts of the mother. The females show a slight preference for the idea that some animals are as intelligent as the average human and for the idea of telepathy. No very convincing explanation of these apparent sex-differences occurs to the author. In general they do not seem to him very impressive or important. Probably the important sex-difference is the tendency of the females to mark a greater number of statements true. On the average, they marked 41.1% of the statements thus, while the men marked 35.1%.

References

(1) E. S. Conklin, Supersititious Belief and Practice among College Students, this JOURNAL, 30, 1919, 83-102.

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ON THE SPECIFICITY OF EMOTIONAL REACTIONS

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In the last decade much discussion has been devoted to the question whether what is called 'general intelligence' is something dependent upon a single general factor or is merely a convenient expression for the central tendency of a number of more or less independent factors or specific abilities. I wish to call attention to the fact that a similar problem exists in explaining the nature of emotion, and that the assumption of one or the other point of view in the case of emotion is of considerable theoretical and practical importance.

The question as it relates to the problem of emotivity may be stated as follows: Does an individual termed "emotional" react in an emotional way to all affective stimuli, or does he tend to react specifically to only certain types of affective stimuli or situations? In the first case we might infer that his uniform responsiveness is due to a single central factor which might be called 'general emotivity'; in the second, we could conclude that the selective responsiveness is due to some specific factors which might be termed specific emotivities, comparable to special abilities or types of intelligence in the intellectual domain.

No experimental data are at hand to enable us to argue for either alternative with absolute conviction. Nevertheless, some results which I have obtained in connection with certain experiments with the psychogalvanic reflex[1] as well as a number of empirical observations seem to show some evidence in favor of the view that individuals tend to be "specifically" rather than "generally" emotional.

In the case of the experiments referred to, the specificity of emotional responsiveness is indicated by the nature of the correlations between the magnitudes of galvanic responses obtained with each of a number of different types of affective stimuli for some 20 *S*s. The experiment consisted of subjecting 20 individuals to the same series of emotional stimuli, ranking them in order of magnitude of response to each of the stimuli employed, and finally calculating the correlations and intercorrelations between the galvanic responses on the various types of stimuli. If there existed such a thing as general emotivity in the sense of a single common factor, we should

[1]D. Wechsler, The Measurement of Emotional Reactions, *Archives of Psych.*, 1925, No. 76.

expect that the average intercorrelations between different stimulus-series would tend to be high, since the type of stimulus employed, providing it had been previously established as being effective in provoking emotional responses, ought not to alter significantly the relative position of the *S* among the individuals with whom he is being compared. A highly emotional individual, for instance, should react with comparatively large galvanic deflections, whether the excitation is that of pain, surprise, or what not. If individuals, however, tended to be specifically emotional, the *S* might react very markedly to sudden noises (surprise or fear) but only moderately or insignificantly to pain. In this case, of course, an individual's relative emotional standing as measured by the galvanometer would change with the type of stimulus employed, and the correlations obtained would be likely to be very low.

Examination of the actual figures obtained shows that, while many of the correlations were fairly high (as much as .81), some were quite low (around .20) or even negative (—.16). In general correlations were high between responses to types of stimuli evoking psychologically similar affective states, but small or insignificant between responses to stimuli evoking different affective states. Thus, the correlation between the responses to the sound of a Klaxon horn and those to a flash of magnesium (both, probably, evoking surprise) was .77; that between the reactions to the same Klaxon and the responses provoked by a pin-prick (pain), .47; but the correlation between the reactions to the Klaxon and the responses elicited by the situational stimulus of being asked to solve an arithmetical problem (giving rise in most *S*s to a feeling of apprehension or expectancy) was only .20.

The above results I take to indicate, for reasons already given, that individuals tend to react in a specific rather than in a general way to emotional excitation; but the results are not offered as unqualifiedly demonstrative of the conclusion indicated. The interpretation of the results is necessarily restricted by the limitations of the experimental method employed. Among these are, first, the relatively small number of *S*s studied and, secondly, the limited range of the stimuli employed. Further experiments are obviously needed to make the general conclusion more cogent. In the meantime, I would call attention to the fact that the conclusion is not unsupported by many instances that might be recalled from general observation. We find, *e. g.*, people who tremble at the sound of thunder but are quite unafraid of most other things. A nurse who will assist calmly at a bloody operation may shriek at the sight of a harmless mouse. The same man who applauds vociferously at a baseball game may fall asleep at the opera. Unless these

instances are exceptions rather than the rule, it would seem erroneous to use the term 'emotional' as implying a characteristic type of reaction. To be correct, we should in each case have to specifiy 'emotional with respect to such and such stimuli' or 'in such and such situations.' This limitation of connotation in the term 'emotional' is in fact assumed when the term is used in a practical way. If a railroad company, for instance, is selecting motormen and inquires whether the applicant is or is not emotional, it is not interested in finding out whether he is much stirred by music, gets quickly excited in an argument, or is readily provoked by his wife; what it wants to know is whether he is readily frightened by sudden noises, tends to lose his head under danger, and the like. And in using the term 'emotional' it restricts it to precisely these specific types of situations which it has in mind.

In summary of the above observations, it would seem that there is much against the assumption of a theory of general emotivity dependent upon a single central factor. On the other hand, there appears to be some good evidence for the view that individuals tend to be 'specifically' rather than 'generally' emotional. But the two conditions are not mutually exclusive. It is altogether possible that what is termed general emotivity may be determined by both a single central factor, analogous to Spearman's "g" in the case of general intelligence, and a number of less important but contributory independent factors which give emotivity its 'specific' character. Evidence for this point of view is, however, not yet at hand. Until it is available, I think that we are more justified and shall find it more useful to speak of specific emotivities than of general emotivity.

THE VALIDITY OF HEYMANS' LAW

By LLEWELLYN T. SPENCER, Yale University

In 1899 Heymans published his general law of inhibition (*2*) which states that the inhibitory power of a stimulus, measured by the intensity of a second stimulus which it can just completely inhibit, is proportional to its intensity. This means that a simple threshold is raised by the presence of a second stimulus in the environment; and that, given the amount of this increase for one stimulus, we may predict by direct proportion the value of the simple threshold when the second stimulus is varied by known amounts. Heymans found this generalization to be true in vision, pressure, taste, and sound.

The experiments which led Heymans to this general law were exceedingly painstaking but were carried on with only a single *S*. Révész (*9*) reported a proportional relationship between the threshold for various colors and the intensity of a surrounding contrast-field of white light. This is evidently a case of the same phenomenon described by Heymans.

J. E. Anderson (*1*) reported experiments on 8 *S*s showing that the differential threshold for two areas of illumination varied proportionally with the intensity of a third stimulus located midway between the areas differentiated.

In 1923 the writer (*10*) reported that Heymans' law held true for 5 albino rats for white light. The intensity of illumination to which the rats just failed in responding with 80% accuracy varied in direct proportion with the intensity of a second light-stimulus.

The raising of the simple threshold for the human being by a second stimulus has thus been demonstrated to be proportional to the latter only by Heymans with one *S*, although his findings are supported by Révész' contrast-work and by Anderson's study of the raising of the differential threshold. The following experiment was therefore conducted in order to test the validity of Heymans' original generalization with a relatively large number of human *S*s.

The apparatus used was a special slide photometer in which 45 watt Mazda lamps were moved to and from the *S* on individual slides running in separate dead-black, light-tight alleys. The position of each lamp was read at the end of the apparatus opposite the *S* from a scale fastened to the slide. The scale was laid out in logarithmic steps, so that equal readings on the scale represented equal variations in intensity of illumination at the aperture from which the *S* viewed the lights. The illumination fell upon a milk glass circular aperture 1 cm. in diam. and viewed by the *S* from a fixed distance of 1 m. When a lamp was advanced to the maximal position near the aperture the scale read 1,000 units. As the lamp was moved away the scale read by logarithmic steps, giving the fractional parts of the 1,000 light-units presented at the aperture. Values given in the results below therefore represent such light-units, *i.e.*, thousandths of the maximal intensity. The lights employed were three in number, in a horizontal line,

with the centers of their respective apertures 6.25 cm. apart. The apertures were all of equal size. As viewed by the *S* the left-hand aperture was screened by a black glass filter to reduce the intensity so that the threshold would fall within the limits of the photometer scale. This light will hereafter be referred to as the *passive* stimulus. The middle aperture was covered with the milk glass only, so that its light, compared to that of the passive stimulus, was relatively intense, although never unpleasantly so. This stimulus is hereafter called the *active* stimulus. The right-hand aperture served throughout as a fixation point and was screened with a ruby glass filter and kept constant at a relatively low intensity. It therefore acted as a constant inhibiting factor comparable, *e.g.*, to the pressure constants of the *S*'s clothing, etc.

The experiment was conducted in a dark room. Fifteen min. for dark adaptation were allowed before any operations were begun. It usually was about 30 min. before any threshold value was fully determined; and as the whole experiment lasted only 60 min. the dark-adaptation factor may be assumed to be approximately a constant, except as varied by the stimulus illuminations as suggested below. After the 15 min. period the threshold for the passive stimulus was determined under three conditions, during all of which the red stimulus to the right remained constant: (*a*) with the middle or active stimulus not present, *i.e.*, =0; (*b*) with the active equal to 5 light-units; and (*c*) with the active stimulus equal to 10 light-units. It should be noted that the light-units in which the active stimulus is measured are not the same absolutely as those measuring the passive, owing to the interposition of the black glass filter before the latter. A light-unit is thus one thousandth of the maximal intensity attainable for the *given* aperture. The use of the red fixation point resulted in the thresholds being obtained for the periphery of the retina.

The procedure used in determining the thresholds was as follows. The passive stimulus was set at 1,000 light-units, *i.e.*, maximal intensity, and the *S* was asked if he could see it. Upon receiving an affirmative answer, the *E* reduced the intensity by fairly large jumps until the *S* reported that no stimulus could be discerned. The stimulus was then returned to the least intensity visible, and the exact value of the threshold was determined by the method of right and wrong cases. A double-throw switch controlled the presence or absence of the passive stimulus. The sound of the throwing of the switch was identical whether the light was present or not. When the *E* threw the switch he gave the signal "Ready," and the *S* fixated the red stimulus and attempted to judge as to the presence or absence of the passive stimulus. Five sec. were allowed to permit of the fluctuation of attention, and the circuit was broken with the signal "Now." The *S* was asked to report as to the presence of the passive stimulus at any time during the 5 sec. interval. Ten such trials were made at a given intensity, and then the next lower intensity used for ten trials, until a report of less than 80% accuracy was made. Between each series of 10 a brief rest was allowed. The threshold was redetermined in the same way for conditions (*b*) and (*c*) as stated above.

The results for the 50 *S*s tested are given below in Table I and Plate 1. A glance at the plate will indicate the rectilinear relationship between the active stimulus and the passive threshold for the group. The figures to the right in Table I are given to indicate how closely each individual approximated a perfect direct proportion. The straight line of best fit for each individual's values was calculated, and his deviations from such straight-line values were tabulated for each of the three thresholds. The means of these deviations for each threshold for the

50 individuals were computed, and are given in the Table as the mean deviations from individual straight lines of best fit. The figures indicate that the deviations from a perfect proportion were in general but fractions of a light-unit.

TABLE I
50 cases

	Mean Thresholds			M. D. from indiv. straight lines of best fit		
Active stimulus 0		5	10	0	5	10
Passive " 11.89		43.84	74.44	0.645	1.25	0.729

Two additional cases may be referred to as Part *B* of the experiment. These individuals had only uniocular vision, owing to traumatic loss of vision in one eye. Their results are given in Table II, together with the straight lines of best fit, for purposes of comparison.

TABLE II
2 cases
Uniocular vision

	Observed Thresholds			Best-fit values		
Active stimulus 0		5	10	0	5	109
S A	9	47	80	9.82	45.33	80.8
S B	9	40	70	9.15	39.66	70.1

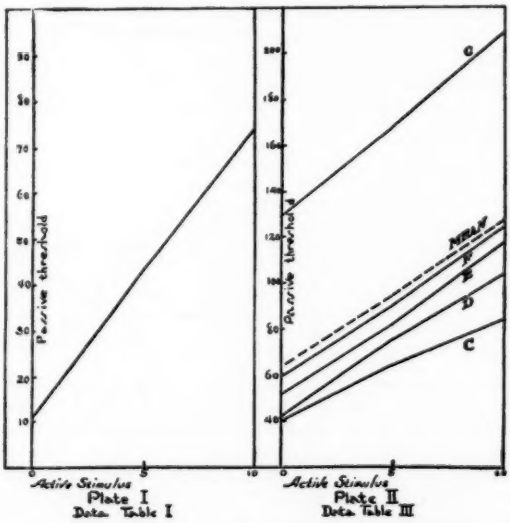
A third variation, Part *C*, was conducted to determine the validity of the law when the active or inhibitory stimulus was presented to the right eye only and the passive stimulus only to the left eye. This procedure should bear on the question whether the inhibitory nature of the phenomenon is peripheral or central, inasmuch as the stimulation and resulting impulses cannot conflict until reaching the optic chiasmus at least. In this experiment the active stimulus was placed at the aperture formerly occupied by the red fixation-point, *i.e.*, the centers of the passive and active stimuli were now 12.5 cm. apart. No fixation-point was used, and the threshold values are presumably for approximately foveal vision. A partition extended from the mid-point between the two apertures to the bridge of the *S*'s nose. Five *S*s served in this experiment. Their results are given in Table III and Plate II. The black filter before the passive beam was inadvertently altered slightly so that the illumination was decreased. This may account in part for the somewhat higher thresholds obtained, as compared to the first and second Parts of the investigation.

TABLE III

5 cases

Passive confined to left eye; active confined to right

	Observed Thresholks			Best-fit values		
Active stimulus	0	5	10	0	5	10
S C	40	65	85	40.79	63.33	85.87
S D	41	75	105	41.65	73.66	105.68
S E	52	82	118	50.96	84.	117.04
S F	59	90	125	58.21	91.33	124.35
S G	129	168	210	128.5	169.	209.5
Mean	64.2	96	128.6	64.06	96.26	128.46



The results of all parts of the investigation uphold the generalization first published by Heymans. The relationship holds for uniocular as well as for binocular vision. It also holds true when the active and passive stimuli fall upon different eyes. The present investigation, then, although it is subject to various sources of error which have not been completely controlled, as suggested below, serves as contributory support of Heymans' generalization. Part *A* of the experiment is significant, especially, because it represents the first confirmation of the phenomena for a relatively large number of cases. The closeness with which not only the mean values but also the individual values conform to a straight-line relationship is also very striking.

The evidence collected in this Study supports the general conclusion that the simple threshold is related to the intensity of a second stimulus simultaneously presented by a direct proportion. This may not necessarily support Heymans in his statement of the phenomenon as one of central inhibition. In order to consider the question of the actual nature of the phenomenon it will be necessary to discuss the sources of error in the present experiment and also other possibilities of explanation which might make the phenomena interpretable upon other grounds than those of central inhibition. Since the question raised concerns not merely the interpretation of the present experiment but also the significance of Heymans' experiments as well, the various critical cases studied by Heymans and others may also be cited as factors in our discussion. Let us first consider other possible explanations of the phenomena.

(1) A physical interference, *i.e.*, the interference of the light-waves before reaching the retina is hardly conceivable as an explanation. Part *C* effectually disposes of this possibility. Furthermore, Heymans' study of pressure stimuli (*3*) where no such interference can occur reveals the same phenomena as in vision.

(2) The disappearance of the passive stimulus when the active is presented may be caused by diffusion of the illumination by the refractive media of the eye, so that the increase in illumination caused by the presenting of the passive may be below the differential threshold for discrimination from the surrounding areas stimulated by the diffused active stimulus. Again, experiment *C* refutes this contention, or at least refers such an occurrence to a theoretical combined retina which is certainly not peripheral. In addition the experiments of Heymans on the inhibition of one mode of excitation by another (*4*), *e.g.*, of sound by electrical stimulation, are not compatible with this view. Jacobson also reports the inhibition of sound by pressure (*6*) and odor by sound and sound by odor (*7*), although his results do not deal with the quantitative relationship which forms Heymans' law.

(3) The phenomena appear akin to those of simultaneous contrast which is known to decrease the apparent brightness of an area contrasted with a brighter area. This explanation may be accepted; but Heymans suggests that simultaneous contrast is really a case of inhibition such as he describes, and by a series of careful experiments shows that the brightness perceived varies from the extreme of negative contrast to the extreme of positive contrast in a regular and continuous manner with the brightness of the contrasting field (*5*).

(4) The phenomena may be considered to occur because of a withdrawal of attention from the passive stimulus. Several

investigators find support for the importance of this factor (*6, 8*). The direction of attention will, *e.g.*, affect the amount of interference or may under circumstances completely counteract it. But attention itself is a form of inhibition—or at least a facilitation at the expense of other processes. Heymans' experiments were carried on with the voluntary attention held constant so far as possible on the passive stimulus.

Each of these alternatives can thus be shown to be inadequate to cover all of the cases studied, or may itself be interpreted as identical with a central inhibitory process. It is not denied that certain of these, *e.g.*, (*2*) under uniocular stimulation, may be partial influences in determining the absolute amount of increase necessary in the passive threshold; but it seems evident that in addition to these possibilities there is an underlying phenomenon identical throughout the various situations studied, which is one of inhibition and at least under certain circumstances is central.

The experimental evidence reported in this paper is not, of course, fully conclusive. Various sources of error must be considered. Each *S*'s thresholds were determined only by a single value although the method used gives such values a fair degree of reliability. Again, the improvement-factor in reacting to very weak stimuli would act to lower the second and third thresholds. The average line for the 50 *S*s of Part *A* does indicate a variation from a straight line in the direction of lower thresholds with the active stimulus equal to 10 light-units, although this variation is, of course, barely more than a single light-unit. The improvement-factor is accompanied by another factor whose effect would be in a similar direction, *viz.*, the increasing dark adaptation of the *S*. As indicated above, this factor is not perhaps particularly disturbing except as noted below. Both improvement and increasing dark adaptation are compensated—although to an undetermined degree—by the effect of fatigue. Although frequent rests were allowed, as already mentioned, it is probable that fatigue can not be neglected as a possible disturbing factor.

The most serious weakness of the experiments as an attempt to test conclusively the validity of Heymans' law as a phenomenon of central inhibition inheres in another aspect of the dark-adaptation factor. It is a possible interpretation of the values herein reported that, by the introduction or increase of the active stimulus, the degree of dark adaptation is altered in such a way as to make a formerly visible passive stimulus now undiscernible. Such a contention may indeed be urged against Heymans' own experiments with light, even though many of his experiments were done with the light-adapted eye. Such an influence is exceedingly difficult to eliminate in visual experi-

mentation. Although such an explanation does not seem sufficient to invalidate Heymans' conclusions with regard to the fields of taste, pressure, and hearing investigated by him, it does prevent us from making any final statement with regard to vision. The present investigation substantiates Heymans' results in vision, but does not finally settle the question whether such results are similar in nature to those which he obtained in other sensory fields. A conclusive answer to this problem remains to be established by further investigation.

Summary and Conclusions

A group of 50 individuals show both individually and collectively that the visual threshold for white light increases in direct proportion with the increase in intensity of a second white light placed 6.25 cm. distant in a horizontal line, a constant red stimulus beyond the second white light serving as a fixation-point. Two uniocular *S*s under the same conditions also confirm the relationship. Five individuals exhibited the same relationship when the threshold stimulus was restricted to the left eye and the second stimulus was restricted to the right eye, the fixation light being discarded and the distance between the stimuli being doubled.

These results confirm those of Heymans and support the quantitative statement of his law. The question whether the relationship found is one of central inhibition is discussed, with the conclusion that other explanations do not seem adequate to subsume all the facts or are not incompatible with the inhibitory view. The desirability of further experiments in vision, especially with the factor of dark adaptation controlled, is indicated.

References

(*1*) J. E. Anderson, *Psych. Bull.*, 21, 1924, 99–100. Cf. this JOURNAL, *infra*.

(*2*) G. Heymans, *Z. f. Psych. u. Phys. d. Sinn.*, 21, 1899, 321-359.

(*3*) *Id.*, *ibid.*, 26, 1901, 305-316.

(*4*) *Id.*, *ibid.*, 34, 1904, 15-28.

(*5*) *Id.*, *Z. f. Psych.*, 41, 1906, 94-100.

(*6*) E. Jacobson, *Psych. Rev.*, 18, 1911, 24-53.

(*7*) *Id.*, this JOURNAL, 23, 1912, 345-369.

(*8*) S. M. Newhall, *Archives of Psych.*, 61, 1923, 75.

(*9*) G. Révész, *Z. f. Sinnesphysiol.*, 41, 1907, 1-36.

(*10*) L. T. Spencer, *Jour. Comp. Psych.*, 3, 1923, 389-408.

THE FIDELITY OF REPORT OF NORMAL AND SUBNORMAL CHILDREN

By JOHN A. MCGEOCH, Washington University

It is still an open question whether or not intelligence conditions the ability to report. It is, at the same time, a question of no small practical importance, as well as one of scientific interest. The most definite statement to be found regarding the dependence of report-ability on intelligence is that of Whipple (1): "There is no conclusive evidence upon the relation between good report and general intelligence." There are, of course, numerous general statements unsupported by evidence.

Breukink's work (2) suggests that intelligence may play a positive part in report, since he found that persons of culture, as represented in the educated professions, gave more extended and accurate reports than relatively uncultured observers, such as nurses and working men. Franken (3) found that his more gifted and cultured *Ss* tended to excel through a greater tendency to caution. V. C. Hicks (4), in a practical case at Vineland, where the veracity of the testimony of two girls about a certain abuse was in doubt, gave the Stanford Revision of the Binet tests and, finding that the I. Q. of each was 75, that neither could repeat the 6 digits of the 10-year group, and that both were lacking in memory and imagination, concluded that they were not capable of making up such a story as they told, and accepted their testimony. Duprée (5) gives a general account of the poor testimony of defectives and warns "emphatically against the peculiar danger that exists when the defect is latent or mild and unsuspected by the court."

The most extensive experimental work on the subject has been done by Pear and Wyatt (6). They took the report, on an event test, of 65 normal children and 78 defectives. They define their defective children as "children of such a degree of mental subnormality that they had been adjudged to require teaching in 'special' schools (in the sense usually attached to the word in England)." Their normal children are those attending regular schools. Such a method for the selection of *Ss* leaves a doubt how defective their defectives were, and perhaps how normal their normals were. Their study indicated that the normals were superior, in report-ability, to the defectives.

The purpose of the investigation here reported was to compare the fidelity of report of two groups of children of known intelligence—one normal and the other definitely subnormal—on three different *Aussage*-tests, the Binet object-card, a picture and an event. The two groups to be compared are from the East St. Louis (Ill.) school system. One is a group of 44 normal children from the Froebel School. The other contains 58 subnormal children from the Ungraded School, to which are sent the most pronounced subnormals of the school system.

The subnormals have an average chronological age of 13.69 years, S. D. 1.75. For comparison with these, a group of very nearly equal chronological age—average 13.81, S. D. 1.00—was chosen from the 8^1-8^2 grades. Each group was given the National Intelligence Tests, Scale A, Form 1. In addition, a second intelligence test was given to each to make more certain that a fair measure of intelligence had been obtained. Scale B, Form 1, of the National Intelligence Test was given to the subnormals, and Army Alpha, Form 5, to the normals.[1]

The mean score of the subnormals on Scale A was 75.70, S. D. 23.35, and on Scale B it was 74.76, S. D. 20.00. The mean score of the normals on Scale A was 125.22, S. D. 16.15, and on Army Alpha 86.90, S. D. 16.55. The norms for year 13^2 are 124 for Scale A and 123 for Scale B. The subnormals, therefore, lie 48.3 points below the norm for Scale A, and 48.24 points below that for Scale B. The normal group have a mean score which is practically at (one point above) the norm. On Army Alpha the normals stand above the norm for year 13, which is variously given as 76, 83, and 84.[3] It thus appears that we are dealing here with one group which is definitely subnormal and another which is definitely normal.

The relative mental status of the two groups may be expressed in a fashion more generally comprehensible, perhaps, if the scores be translated into mental ages, although this procedure is not strictly accurate. So translated, the National Intelligence Test average mental ages are, for the subnormals, 10 years 6 months on Scale A, and 9 years 11 months on Scale B; and for the normals 13 years 7 months on Scale A. Interpolating Alpha scores into mental ages is dangerous, but as it stands the average Alpha score of the normals gives them an average mental age of somewhere between 13 and 14 years at least. In terms of mental age, therefore, we are dealing with two groups which, although practically equal in average chronological age, differ by at least 3 years in mental age. In so far as the average indicates, the classification of the subnormal group is borderline deficiency. This group included, however, a few very feeble-minded cases and a few who are low normals. In the normal group there are a few low normals, but when the distribution curves for the two groups are drawn, they overlap scarcely at all. We have, then, for comparison two groups of distinctly different mental status.

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[1]Army Alpha was chosen as a second test for the normals at the request of the principal of the school.

[2]Given in Supplement No. 2 to the Manual of Directions for the National Intelligence Tests, Table 4.

[3]Given in the Manual of Instruction for the Army Alpha Intelligence Tests, prepared by J. C. De Voss.

With each of these groups three different report-experiments were conducted.

The material for the first was the Binet object-card,[4] used by Binet (7) in some of the early experiments in the psychology of report. It is a rectangular sheet of orange-yellow cardboard, 33.5×40.5 cm. Attached to it are two photographs, a red and white label, a white button, a penny and a one-cent postage stamp. The *S*s were given the following directions: "I want to find out how much you can see and how much you can tell me about what you have seen. I am going to show you a large piece of cardboard with several objects fastened on it. You will have just thirty seconds, or half a minute, in which to look at it. Half a minute is a pretty short time, so you will have to look pretty hard, because afterwards I shall ask you to write out a description of what you have seen; and then I shall ask you some questions about what you saw, and I want you to write the description and answer the questions as accurately as you can." The *S*s were then shown the card for 30 sec., at the end of which time the card was again covered. The instructions for the writing of the narrative or description were: "Write out an account of everything you saw; describe it so clearly that, if I had never seen the card, I should know all about what was on it. You will have as long a time as you wish in which to do it."

When the narrative was completed, the *S* was given a mimeographed sheet containing 50 questions, with spaces after them for the answers, and was told to answer the questions. The questions were, with two or three exceptions, those suggested by Whipple (1) for use with the object-card.

The material for the second experiment was the picture entitled "A Disputed Case," 15 × 17 in. The objects represented in it are practically all of good size, practically all colored, and are not too numerous to be carefully noticed in the time allowed. The directions for this were the same as those for the object-card, except for the modifications necessary with the changed material. The children were not, however, told how long they were to have to look at it, but were told only that it would be a "short" time. The time allowed was 20 sec. The narrative was taken, followed by an interrogatory of 50 questions taken mainly from Whipple (1).

The third experiment was an event after the fashion of those used by Jaffa in von Liszt's seminar at Berlin (8), by Lipmann (9), by Morgan (10), McGeoch (11), and Pear and Wyatt (6). Two persons participated in the event, the conditions of which had been prearranged with the teachers of the various rooms. The persons were a student, skilled in amateur dramatics (K), and the writer (M). The children had never seen the former, but had seen the latter several times before, when he was giving the intelligence tests and the two report tests mentioned above. On the day of the event, however, he was dressed otherwise than usual.

While class was in progress, M knocked at the door and entered the room, carrying a dark-brown Boston bag, followed by K, attired in brownish grey knickerbockers, with light grey stockings and tan oxfords, and carrying a raincoat and an umbrella. Just as both were clearly inside the door, they exchanged the articles they were carrying. Then K turned to the teacher and said, "Are you the teacher of this room?" She replied "Yes." Then K walked to the desk, placed the bag on top of it, in plain sight of all the children, and began taking out of the bag several articles—a red book, a number of sheets of paper (the interrogatory blanks, which were left on the desk), a light grey cap, and a book with a yellow cover and blue back, called the "Slant Book," its cover being cut on a slant, so that when it is open it looks like a pair of butterfly wings, with a large V

[4]The modification of the original Binet object-card furnished by C. H. Stoelting Co. was used.

at the top. The articles were held up for a moment before being deposited on the desk, K giving the impression that he was searching for something. When at last he came to the "Slant Book," he opened it and held it out so that the teacher could look into it, its back being toward the class. Then he said, "I have here a new book in arithmetic. Would you like to buy some for the class?" The teacher replied, "No, not to-day." K bowed and slowly replaced in the bag each object which he had previously taken out, with the exception of the papers, but in reverse order. He then took up the bag and walked out, followed by M, who had been doing nothing, during the interval, except to stand where he had stopped upon entering the room, being careful to hold the umbrella and raincoat in plain sight. As it was a clear day in spring, these articles were somewhat out of the ordinary. M carried a stop-watch, which was started upon entering the room and stopped as he left. The time for the event before each of the groups was about 40 sec., with only 2 or 3 sec. variation in any case. The event was made brief, in order to keep it from becoming complicated, and to make it conform in time to the brief occurrences upon which children are often called to report in everyday life.

As soon as the participants had left the room, the teacher asked the children to write out an account of everything that had been said and done, according to written directions which had been given to her previously. An interrogatory of 25 questions was used. In general the procedure was the same as for the object-card.

With each of the three types of material, both narrative and interrogatory reports were taken, and the children were given unlimited time in which to write the narrative and to answer the questions. These were, therefore, of the nature of "power" tests, the element of speed being eliminated. Each type of material was given as a group test. There were three groups of the subnormals and two of the normals. The event was enacted in the front of the room and in full view of each group. With the two other types of material, the *Ss* were told to come two at a time to a table in the front of the room, where they were allowed to look at the material for the time allowed—30 sec. for the object-card and 20 sec. for the picture. Immediately afterwards they were told to start writing the description. This method made it possible to give the test to all the members of a group in a comparatively short time. Each type of test was given to all the *Ss* in the same building on the same day and in such a fashion that there could be no communication between groups in the meantime. The Froebel School and the Ungraded School are sufficiently far apart to render intercommunication regarding the tests highly unlikely.

In one part of the experiment, with the picture, the members of each group were asked to underline each item of the narrative of which they were so absolutely certain that they would be willing to take oath in court that it was correct, the meaning of such certainty being further explained to them. In the interrogatory they were asked to circle the number of each question of the answer to which they were certain. This procedure was followed in only one part of the experiment with the subnormal group, because the extra time involved in going back over narrative and interrogatory to check the items of which they were certain rendered it impracticable to carry it further. Even after they had performed previously with the object-card and were therefore somewhat used to the general method, the attestation on the picture test consumed a large amount of time.

The narrative for each test was scored by counting each separate item, right or wrong, as one. Interpretations and indefinite statements were not counted. The interrogatory was scored by counting the answer to each question, right or wrong, as one. It would be better if some qualitative method of scoring could be used to supplement the quantitative, but no such method appears to be feasible. As it is, the quantitative score represents with accuracy the absolute fidelity of report and the range of report,

together with items incorrectly reported, without taking into account the fact that some items are of more value in a report than others.

The prevailing method of taking reports in the past has been to have the *S* give his report orally to the *E*, while the latter writes it down. This consumes a great deal of time. The method of having written reports makes possible the gathering of a much wider range of data than would be possible if each narrative and deposition had to be recorded by the *E*. It is doubtful if there is any decrease in the value of written reports; but if there is, it is more than compensated for by the greater number of cases that can be investigated.

To discover the relative worth of oral and written reports, and to ascertain whether or not the written method could be safely used, 31 oral and 27 written narratives and depositions were taken at the Ungraded School, at the beginning of the investigation. The method used in selecting the *S*s for the oral reports was to take one pupil at random to start with. As soon as he had finished the test, he was told to go back to the room and tell someone else to come to the office where the testing was being done. This was repeated with each *S*, previous arrangements having been made with the teachers to permit it.

When the oral testing, which occupied several afternoons, had been completed, the children who had not been tested were collected in two groups and given the object-card test, this time the reports being written. It would seem that by the method used a fair random selection has been obtained. Table I contains a comparison between the two methods in the number of items correctly and incorrectly reported, together with the total number of items reported. Scores are averages in all cases.

TABLE I

Binet Object-Card

Narrative	*Cases*	*Right*	*Wrong*	*Total*
Oral	31	14.1	2.5	16.6
Written	27	14.5	2.4	16.9
Interrogatory				
Oral	31	27.1	12.1	39.2
Written	27	25.9	13.4	39.3

No very significant differences appear between the results obtained by the two methods. The reports with each method are practically equal both in range and in accuracy. Consequently, written reports were used exclusively throughout the rest of the experiment. Figures for the object-card with the subnormals cited hereafter are for both oral and written reports, as given in Table I.

In Table II are given the comparative scores for normals and subnormals on the object-card; in Table III the score on "A Disputed Case"; and in Table IV those for the event.

TABLE II

Comparative Scores on Binet Object-Card

Normals 44 Cases	*Right*	*Wrong*	*Total*	*% of error*
Narrative	22.9	3.5	26.4	13.2
Interrogatory	32.2	11.7	43.9	26.6
Subnormals 58 Cases				
Narrative	14.3	2.5	16.8	14.8
Interrogatory	26.7	12.6	39.3	32.0

The range of report (total number of items reported) is greater for the normals in both narrative and interrogatory; the difference being 9.6 on the narrative and 4.6 on the interrogatory. In number of items rightly reported the normals average 8.6 higher on the narrative and 5.5 higher on the interrogatory. For both groups the range is greater on interrogatory than on narrative, and the percentage of error is slightly more than twice as great for the interrogatory. The percentage of error is greater throughout for the subnormals. It should be pointed out, however, that in actual number of errors there is no very great difference between the two groups.

For the picture, attested and unattested items are shown separately. Averages for these items include only those cases who gave attested or unattested items. Some *S*s attested all items; some failed to attest any. When the averages take this fact into account, they are more significant with respect to the accuracy and range of attestation and the lack of it. However, this makes the total averages given fail to agree with the total of the averages for right and wrong items. This must be borne in mind, when the Tables where attested items are given are interpreted.

TABLE III

Comparative Scores on "A Disputed Case"

		Normals			
Narrative	*Cases*	*Right*	*Wrong*	*Total*	*% of error*
Attested	37	28.0	4.3	32.3	13.3
Unattested	36	5.2	3.3	8.5	38.8
Total	39	31.4	7.1	38.5	18.3
Interrogatory					
Attested	35	18.0	10.4	28.4	36.6
Unattested	39	9.7	11.6	21.3	54.4
Total	39	25.9	20.9	46.8	44.6
		Subnormals			
Narrative					
Attested	27	12.0	2.5	14.5	17.2
Unattested	35	8.0	1.5	9.5	15.7
Total	41	14.8	2.9	17.7	16.3
Interrogatory					
Attested	14	10.8	7.1	17.9	39.6
Unattested	41	16.6	14.3	30.9	46.2
Total	41	20.3	16.6	36.9	44.9

The range of report of the normal group is greater than on the object-card in both narrative and interrogatory. For the subnormals it is slightly higher on the picture narrative and slightly lower on the picture interrogatory. Here also the normals excel the subnormals in range of report, this time by 20.8 on the narrative and by 9.8 on the interrogatory. In number of items rightly reported the normals excel also. In percentage of error there is a slight balance in favor of the subnormals on the narrative and a practically equal percentage on the interrogatory. In both groups it is high on the latter. The much greater range of the normal group is a significant advantage; but in so far as the actual number of errors is concerned, the subnormal group is superior, having fewer actual errors than the normals.

When attestation is considered, it is seen that the normals attest more items and with a smaller percentage of error than the subnormals on the narrative. The normals, in fact, attest the majority of the items, and the percentage of error in the remaining and unattested items is remarkably high. It is significant that the percentage of error in the items unattested by the subnormals is smaller than that for the attested items. It would not appear that attestation with them is any more an assurance of accuracy than lack of attestation. The same general conclusions hold true for the interrogatory, except that there the subnormals have a smaller percentage of error in attested than in unattested items.

TABLE IV

Comparative Scores on Event Test

	Right	*Wrong*	*Total*	*% of error*
Normals 41 cases				
Narrative	45.0	6.1	51.1	11.9
Interrogatory	13.8	8.9	22.7	39.2
Subnormals 31 Cases				
Narrative	26.9	6.6	33.5	19.7
Interrogatory	11.5	10.0	21.5	46.5

Inspection of the above Table shows that, for both groups, the range of report in the narrative is higher on the event than on either object-card or picture. The children grasped more details and described with more minuteness than on either of the two other tests.

The range of report on the event is uniformly higher for the normal group, being higher by 17.6 on the narrative and 1.2 on the interrogatory. It should be remembered that in the interrogatory for the event test there were but 25 questions. The percentage of error is greater throughout for the subnormals, and for both groups it is larger by far on the interrogatory than on the narrative. The subnormals also have a greater average number of actual errors.

The sigmas of all the differences, hitherto cited, between normal and subnormal groups are small enough to indicate that the differences are significant. In every case the difference is at least three times the sigma, and in most cases is much more than three times the sigma.

An analysis of the number of individuals giving errorless reports shows that there were a few such reports among the narratives, but none at all among the interrogatories. The percentages of individuals giving errorless reports, together with the comparative ranges of their reports, are given in Table V. Under "Range" the first figure means average number of items given by those having errorless reports; the second figure means average number of items reported by the remainder, whose reports showed some error. The same thing is shown for attested items, reported on the picture, in Table VI.

TABLE V

Errorless Reports

	Object-Card	*Range*	*Picture*	*Range*	*Event*	*Range*
Normals	.06	17.0–22.4	.05	24.5–39.4	.02	16.0–52.0
Subnormals	.22	15.9–17.1	.07	9.6–18.4	.06	16.5–34.6

TABLE VI

Errorless Reports—Attested and Unattested Items

	Attested	*Range*	*Unattested*	*Range*
Normals	.09	20.2–33.8	.16	2.8– 9.8
Subnormals	.11	9.3–15.2	.25	6.4–10.5

On all the narratives for both groups a small percentage of errorless reports was found. It is highest in the case of the subnormals with the object-card. When the attested and unattested items reported on the picture are taken separately, it is found that for both groups there is a smaller percentage of errorless reports in attested than in unattested items. Throughout, however, the range of those giving errorless reports is much smaller than that of the others. A strictly accurate report, therefore, seems necessarily to be less extended. The subnormals give a greater percentage of errorless reports than the normals; but when their greatly lessened range of report is considered, it may be concluded that they are not, on the whole, as good reporters.

Coefficients of Correlation.—From the foregoing data it seems established that the normal children give a much wider range of report and a smaller percentage of errors than the subnormals; and that, when they attest their reports, their reliability of oath is greater than that of the subnormals. It must be remembered, however, that on the object-card and the picture the subnormals have a smaller average number of actual errors than the normals, with the one exception on the object-card

interrogatory. As a general conclusion, therefore, it can be safely said that normality of intelligence positively conditions range of report, percentage of accuracy and reliability of oath. It is worth while, however, to inquire more specifically what is the relationship between fidelity of report and intelligence, within the groups. The coefficients of correlation between *Aussage*-scores and scores on the National Intelligence Tests are presented below. The product-moment method has been used.

TABLE VII

Coefficients of Correlation

National Scale A with	Narrative		
	Normals		
	No. Right	No. Wrong	Total No.
Object-Card	−.165±.103	−.063±.105	−.149±.103
Picture	.000±.115	−.111±.113	.000±.115
Event	.028±.115	−.216±.109	−.028±.115
	Subnormals		
Object-Card	.364±.081	.159±.091	.331±.083
Picture	.424±.077	.335±.095	.396±.079
Event	.413±.104	.164±.120	.393±.106
	Interrogatory		
	Normals		
	No. Right	No. Wrong	Total No.
Object-Card	.030±.109	−.044±.105	−.092±.108
Picture	.223±.108	−.277±.106	−.118±.113
Event	−.181±.111	−.011±.115	−.196±.100
	Subnormals		
Object-Card	.216±.089	−.141±.092	.172±.091
Picture	.578±.062	.204±.102	.530±.067
Event	.435±.102	.289±.113	.486±.096

The coefficients of correlation between Scale A *plus* Scale B and report, in the case of the subnormals, and between Scale A *plus* Army Alpha and report, in the case of the normals, have been computed, but they are so nearly the same as those given above that it is hardly worth while to give them here. Their computation, however, is valuable as affording a check on the accuracy of the intelligence measurement, and gives added assurance regarding the validity of the results.

Some very unexpected results emerged when the intelligence scores and the results of the report experiments were correlated. For the normals the relationships are either slightly inverse or practically zero. In most cases the probable error is nearly as large as, and sometimes larger than, the coefficient of correlation. There is a very slight tendency for the coefficients between intelligence and the number of items rightly reported to be somewhat higher than those between intelligence and the total number of items given, and more often positive. Coefficients of correlation between intelligence and the total number

of items given are slightly negative throughout for the interrogatory, and with the exception of one zero coefficient the same thing holds true for the narrative. The relationships between intelligence and number of errors in report are slightly inverse throughout.

The coefficients of correlation for the subnormals are, with one exception, all positive and indicate a moderate degree of relationship between intelligence and ability to report. Except for the event interrogatory, the coefficients of correlation are higher between intelligence and total number right than between intelligence and total number of items reported. Thus, for both groups, ability to report correctly is slightly more related to intelligence than is the ability to give a wide range of report. There is also some tendency for errors and intelligence to be related positively in the subnormal group. Table VII shows a tendency for the amount reported to increase as intelligence increases up to about normality, above which intelligence differences have little effect. The data of Tables V and VI show that freedom from error and limited range of report go together. The scatter diagrams for the relations between errors and total number of items reported show a low relationship with the normals and a moderately high one with the subnormals. In each case the relationship is greatest at the lower ends of the distributions. The slight positive correlation between errors and intelligence among the subnormals seems accounted for, therefore, by the fact that range tends somewhat to increase with intelligence and errors tend to increase with increase of range. With the normals, just as range and intelligence do not tend to vary together, so neither do errors and intelligence, nor range and errors.

Inspection of the scatter diagrams shows that, at the lower end of the distribution scale of intelligence, report and intelligence vary together much more closely than in the normal ranges of intelligence, and the coefficients indicate a moderate positive correlation in the case of the subnormals and a practically zero relationship in that of the normals. Thus, when all the *Ss* are normal or better, individual variations, even those well above the norm, do not greatly affect report-ability; while, when all are subnormal, the deeper the subnormality, the greater is the tendency to poor report. This may be analogous to the situation with respect to intelligence and college grades, where the intelligence tests can predict marked failure or marked success, but cannot so definitely place the individuals who rank between. There were no markedly supernormal children in the normal group, so that the analogy is not complete, but it may hold for the subnormal cases. Inspection of the scores of the subnormals indicates this conclusion. Or, to put it differently, a very poor level of intelligence may render an

individual unable to report either very much or very accurately; but when one begins to consider normal individuals, whose intelligence level lifts them above the *report-threshold*, as it were, individual variations in intelligence cease to be related with variations in report-ability.

Conclusions

(1) The comparative results obtained from oral and written reports indicate that they are of practically equal value in both range and accuracy, and that the method of taking written reports from children can be used with safety.

(2) The range of report, on object-card, picture and event, is greater for the normals than for the subnormals.

(3) The normal group shows, on the whole, a smaller percentage of report-error; but in the actual number of errors there is no outstanding and consistent difference between the two groups.

(4) When attestation is asked for, the normals attest more items and with a smaller percentage of error than the subnormals, on both narrative and interrogatory.

(5) Both groups describe the event with more minuteness, with a greater range of report, and with more apparent interest, than in the case of either object-card or picture. The event test would, therefore, seem to be the better type of *Aussage*-test for children.

(6) No errorless reports were found in any interrogatory. In the narratives of both groups, on all three kinds of report-test, a small percentage of errorless report was found, but the range of the errorless reports is uniformly less than that of the reports in which errors appear. The subnormals give a greater percentage of errorless reports, but with a more restricted range.

(7) The correlations between intelligence and report-ability, and intelligence and errors in report, show a tendency to zero or inverse relation for the normals, and a tendency toward a moderate positive relationship among the subnormals.

(8) Within the limits of the material and *S*s of this investigation, it may be concluded that intelligence is positively related to report-ability, in the sense that a certain degree of normality of intelligence is necessary for a normal range and accuracy of report; but that, when this degree of normality, or *report-threshold*, is present, there is no definite relationship between intelligence and ability to report.

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A DEVICE FOR THE DEMONSTRATION OF APPARENT MOVEMENT

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Because of its intrinsic scientific interest, and its significance for systematic psychology in general and for the *Gestalt*-psychology in particular, the phenomenon of apparent movement has been the subject of much experimental study in recent years.[1] An exposition of this phenomenon is difficult without visual demonstration. The apparatus described below makes possible such a demonstration to a large class from the lecture table in a normally lighted room.

In this device certain lighted but immobile geometrical forms may be shown in such a way as to give the "illusory" appearance of movement. The following conditions, each of which plays a part in the production of this apparent movement, may be varied independently in the apparatus: (1) the distance between stimulating forms; (2) the intensity of the illumination of the stimulating forms; (3) the temporal interval between the presentation of the forms (the perception here varies from apparent discreteness and succession, through good illusory movement, to apparent simultaneity); (4) the single or continuous presentation of the stimulus-forms; and (5) the influence of the presence or absence of additional neighboring stimulus-forms.[2]

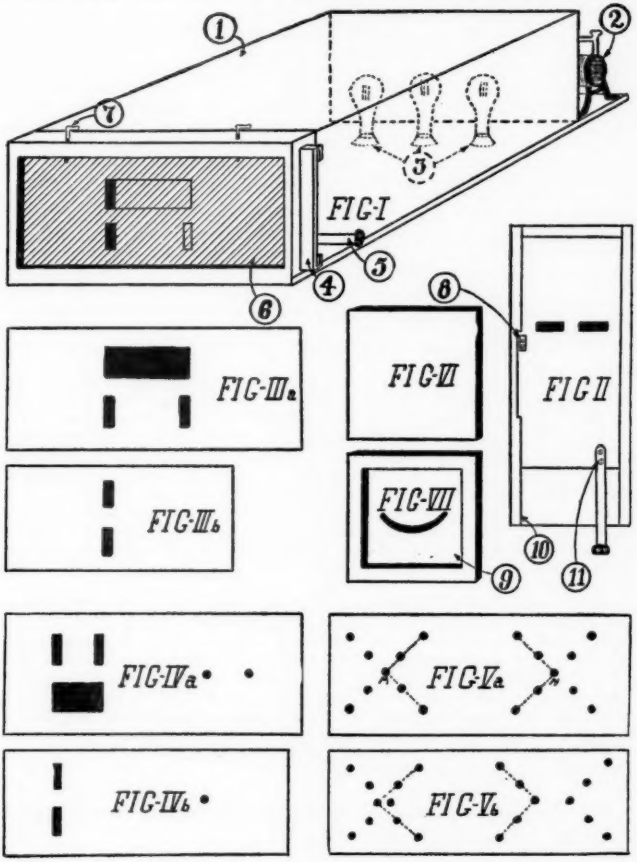
The accompanying diagram will assist in making the nature of the apparatus clear. Fig. I shows the complete device. It consists of a wooden box (1) which is 60 cm. long, 50 cm. wide, and 20 cm. high. At the back of the box is a rheostat (2) by means of which the current to the three 100-watt lamps (3), and hence the intensity of illumination, can be varied. In the front part of the box is a slot in which the slide-units are inserted. The tip of such a slide-unit (4) is seen in the diagram. The back view of a complete slide-unit is shown in Fig. II. The sliding member of such a unit (11) can be moved when the whole slide-unit is in place in the box by means of a handle (5). The slide and the front face of the slide-units are both made of black bakelite, 1/16 in. thick. The grooves are machined in members which are made of 1/4 in. stock of the same material. These members containing the grooves are fastened to the front face by screws. The total movement of the slide in these grooves is rigidly fixed by a brass "bumping plate" (8). The dimensions of the face of the slide-unit are 17 cm. by 40 cm.

Immediately in front of the slide-unit, when it is in place in the box, is permanently fixed a milk-glass or other translucent but not transparent screen (6). A ledge is provided in front of this translucent screen into which an opaque shield, Fig. VI (drawn slightly larger than scale), may be

[1] For a partial bibliography on this subject cf. P. Cermak and K. Koffka, Untersuchungen über Bewegungs- und Verschmelzungs-phänomene, *Psychol. Forschung*, 1, 1921, 127 ff. For later titles cf. the references given by W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, die durch Sukzessiv-Darbietung zweier Reize begrenzt werden, *ibid.*, 5, 1924.

[2] For a treatment of these factors and a relationship between certain of them, cf. A. Korte, Kinematoskopische Untersuchungen, *Z. f. Psychol.*, 72, 1915, 193 ff.

fastened by means of thumb-screws (7). A frame, Fig. VII, holds a second translucent screen made of paper. On this paper screen may be placed certain opaque figures or *Nebenreize* which influence in an illusory manner the perception of the other presented stimulus-figures. This frame may be fastened in the front ledge in the same manner as the opaque shield.



Figg. III, IV and V suggest forms in which the slide-units may be made. In each case the figure with the subscript *a* is the front face, while that with the subscript *b* is the sliding member of the unit. In Fig. II the sliding member (11) is seen in place.

In Fig. III, and in the left design of Fig. IV, the stimulus-forms are so placed that real and apparent movement can be compared at the same

time. Thus, when the device is prepared as shown in Fig. I, pulling the handle (5) will cause the lighted stimulus-forms (shown in solid black), which are each 4 cm. by .5 cm. in size, to move across the translucent screen. The upper lighted form, showing real movement, will travel continuously across the 10 cm. slot. The lower stimulus-form, on the contrary, will be extinguished during the time required to move the 10 cm., until it flashes out again at the extreme right.

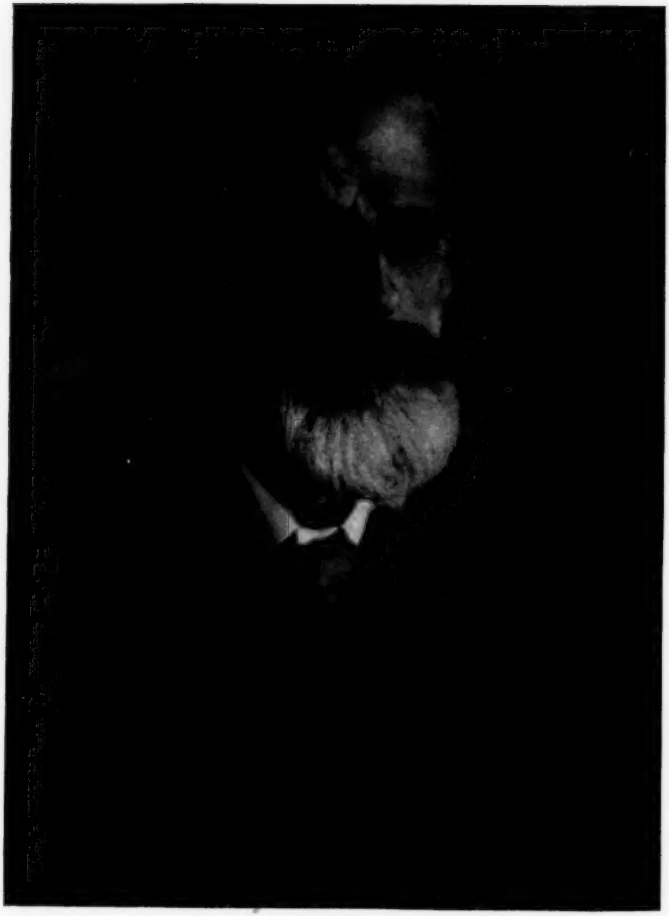
This device makes a most convincing demonstration; for under proper conditions of speed and illumination the *O* can seldom distinguish the real movement of the continuously seen member from the apparent movement of the two non-moving flashes. When the slide is moved slowly, however, the two discrete figures give no suggestion of movement, but appear merely as successive spatially separated light-forms. Similarly, when the slide is moved very fast, the two stimulus-forms are seen without motion, and simultaneously present. The distance between the stimulus-figures may be varied by inserting different slides into the box. Thus, there is a distance of 5 cm. between the forms shown on the left side of Fig. IV, while the comparable distance in Fig. III is 10 cm. When the left side of Fig. IV is to be observed, the right side is covered with the shield shown in Fig. VI.

The design on the right side of the slide-unit shown in Fig. IV under proper conditions gives the illusion of the movement of a circular light of 6 mm. through a straight path of 5 cm. By adding the translucent screen of paper, Fig. VII described above, this straight path will apparently be deflected and most *O*s will report the movement of the light through an arc. This experience may be compared to that of a marble rolling in a bowl.

Fig. V shows an adaptation of the Müller-Lyer illusion. The oblique or acute angles of the "wings" are shown upon the translucent screen by means of circles of light which are shown in black in the diagram. The circles which produce the acute angles when the slide is in position under the front face are shown conventionally by broken lines, which, of course, do not appear in the apparatus. When the slide is moved, the circles M and N appear to have first acute and then oblique "wings." If a fixation point is taken midway between M and N as the figure is changed the dots M and N, although motionless, will appear to jerk back and forth through a considerable distance. Different fixation-points give different illusions as this slide is moved; for example, the "wings" themselves may be seen to fold in and out.

Some little skill is required to move the slides in the various units at the desired speed, and for more exact work this factor might well be mechanically controlled by an electric motor. For determining new designs for the apparatus—those suggested here are merely samples of the large number that have been used in the work upon apparent movement and the "phi phenomenon"—small cardboard models similar to the slide-units described above may be made. Such little models give excellent apparent movement. The fundamental idea, indeed, for the apparatus described here was derived from a simple cardboard device of this kind which is described and figured by Wertheimer.[3]

[3] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Z. f. Psychol.*, 61, 1912, esp. 262-265.



Yours sincerely
James Ward

JAMES WARD 1843-1925

By F. C. Bartlett, Cambridge University

On a Saturday afternoon in February 1914 a number of the late Professor Ward's pupils, colleagues and friends made him a public presentation of his portrait. No one who was there is likely ever to forget the address by which he acknowledged the gift. It was almost wholly autobiographical. Many men cannot tell, for they do not know, the truth about themselves. But I think that all of us who sat in that Cambridge lecture room, in the growing gloom of the late afternoon, and listened to him as he spoke in a perfectly simple, straightforward and unpremeditated way, knew that Dr. Ward saw clearly, and was able to show to us, the springs of his life and of his own activity.

He was a delicate boy and was early away from school. Then, often alone, he wandered about among the sand-hills of the Lancashire coast—he always said that few good things came from south of the Trent—watching the gulls wheel, their wings flashing in the sunlight, and searching the sand-dunes for the curious forms of life that lie hidden there. So there grew up in him a love for nature and wild life that not only remained strong in him to the end, but that had very much to do also with his philosophy. He was, indeed, a first-rate field naturalist, and he did not believe that people could write truly about things of the spirit which cannot be seen, if they knew nothing of the things of life that lie open to all men's vision. Later he took many walking and climbing holidays into the remoter parts of England and Scotland to see wild things in their homes. He made pets of all kinds of living creatures, insects, birds and beasts. Several generations of Cambridge students knew the famous "Jan," a collie dog with a beautiful head, who could "almost speak." Ward was equally enthusiastic about flowers and trees, and made a beautiful garden and worked much in it. The love of nature as it grew stimulated an intense interest in all observational and experimental science. It was this which made him hesitate for long between a career given to the study of life and one devoted to the study of mind. And when he made his choice, it was this which took him first of all to psychology.

James Ward's first venture into serious life was when, still rather young, he entered an architect's office in Liverpool. But he was not very happy there. He started, and did a great deal to maintain, a debating society among his colleagues. They

talked about all the usual subjects, and some unusual ones: Is Logic a Science or an Art?, various educational questions,—and once Ward indicated the trend of his political development by stoutly upholding universal suffrage for women. They said to him: "You are not much of a draughtsman, but you have the gift of the gab. Why not be a lawyer or a parson?" And a parson he determined to be.

He became a student at the Congregational Training College which was then at Spring Hill, near Birmingham, and remained there nearly six years. His study of theology stimulated and developed the philosophical interests which were later to become dominant in his life. Towards the end of his student career he came to Cambridge to preach at the Congregational chapel and was invited to its pastorate. But about the same time he obtained a studentship which gave him the chance of study in Germany, and he proceeded to Berlin and later to Göttingen. There, although he continued to pursue his theological studies, he became more and more interested in physiology and philosophy, and worked at the first with Ludwig, and at the second with Lotze.

Lotze attracted and influenced him profoundly. The wide scientific interests and attainments of the elder man, his power of keen psychological analysis, his constant reiteration of the fundamental importance of life and activity, and the ethical strain underlying his philosophy all appealed strongly to the personality of Ward and became an abiding force in his own life and thought.

In due course he returned to England, to find the Cambridge pastorate still open; and the invitation to him to fill the post was renewed. He accepted with much hesitation, feeling that he no longer held orthodox religious views; and he refused to be ordained. For nine months he was a Nonconformist minister. Again he was not happy. His theological views were far in advance of those of his congregation and he was too intellectually honest to attempt any disguise of the difficulty. He resigned the pastorate, and became a Non-Collegiate student at the University. Then he learned that an open scholarship in "Moral Science" was offered by Trinity College. He entered for the examination, won the scholarship, and thereafter his work lay mainly in academic circles.

This was in 1872. In 1874 he became M. A. of London University, winning the gold medal in philosophy. The year following he was placed by himself in the first class of the old Moral Sciences Tripos at Cambridge, and in the same year was elected to a Fellowship at Trinity College. In 1881 he became a Lecturer at Trinity, and in 1897 was elected to a Chair in

Mental Philosophy and Logic, a post which he held until his death on March 4th, 1925.

This is the story which he told, lingering over the early part of it, hurrying over the second and academic part, on that late afternoon in February. "What I am," he said at the end, "two men have made me: Hermann Lotze and Henry Sidgwick."

This is not the place, and I am not the writer, to attempt to say anything in detail about Ward's philosophical and metaphysical teachings. But it is an interesting thing to consider the course of his development as it is indicated by the Bibliography of his published writings to that date which was printed in this JOURNAL in 1912.[1] The earliest papers dealt, all of them, with biology, physiology and experimental work in psychology. As early as 1882 his interest seemed to be turning more towards problems of general psychological theory, and the few years following that saw the development of his complete psychological system: the famous *Encyclopaedia Britannica* article was first published in 1886. In 1896 he began his first period as Gifford Lecturer, and his work became more metaphysical in its bent. From then, to the conclusion of the period covered by the published Bibliography, philosophy in the broadest sense took up more and more of his thought and energies. In the end it absorbed him almost wholly, so that some years before his death he even gave up lecturing upon psychological topics, and confined his public lectures to metaphysics. The history of his work remarkably exemplifies the nature of his outlook upon the world. The study of life is a stepping-stone to the study of mind; the study of the facts of mind is a stepping-stone to an all-embracing philosophy. The last without the two first is barren, the two first without the last are incomplete.

This view is foreshadowed even in the dissertation which helped to win him his Fellowship at Trinity College in 1875. A part of the thesis was published later in a privately printed essay on *The Relations of Physiology and Psychology*. The work is said to have caused his referees very great heart-burning. They knew and could make nothing of its subject-matter, but were broad-minded enough to see that Ward ought to be given a chance to pursue his interests. It was, in fact, chiefly concerned with the development, by Weber and Fechner, of an experimental and quantitative psychophysics. Ward expounded and criticised—mainly expounded—their work with remarkable clarity and incisiveness, and with unmistakable enthusiasm. Thus he began his official career in Cambridge by a

[1] 23, 457-460.

study of that intractable problem which, though in his case it was approached through the avenues of science, almost inevitably leads to philosophical speculation, that of the relation of body and mind. And every student who has attended his lectures on psychology knows with what fascination Professor Ward again and again approached that subject. There was another part of psychology which had for him a particularly irresistible attraction: the study of memory. And in both cases the underlying interest seemed to be the same. Here, if anywhere, was to be found actual evidence of the integrity and independence of mind, and of the indissoluble character of individual experience.

It is this philosophical motivation, underlying almost every part of Dr. Ward's psychology, that makes it peculiarly difficult to estimate his exact position in relation to contemporary psychology in this country. On the face of it, it would perhaps appear that those psychologists whose mode of approach is scientific rather than philosophical have tended to ignore his work, and have remained but little influenced by his thought. Undoubtedly he appeared somewhat antipathetic towards recent movements in psychology. He regarded the mass of experimental work as on the whole chaotic, more often than not ill-planned, and directed by no great and governing ideas. The tendency to preoccupation with the psychopathological also seemed to him to be definitely misleading and dangerous. But I believe it true to say that, to a degree very rare indeed within the limits of a man's own life, a great deal of his most important work was assimilated, and passed so completely into current ways of thinking that its source often remained unappreciated. For example, his onslaught upon atomism in psychology had a more complete and far-reaching influence in scientific than in philosophical circles. But because he still continued to talk predominantly in terms of presentations, because he remained far more interested in the materials of mental life than in the reactions themselves, because, in spite of his activism, the bulk of his discussions concerned the nature of the content of intellectual processes, scientific psychologists have often tended to look upon him as an intellectualist who treated all too lightly the actual springs of conduct.

Thus views which in some respects look as if they might have come straight out of the pages of his *Encyclopaedia* article or of the *Psychological Principles* have been put forward as if they are quite new. Nowadays most psychologists tend to lay stress upon functions and their conditions. But it was he who, more than any other English thinker, insisted upon the importance of "the subject acting." He was alive to the significance of bias, interest, specific reaction-tendencies of different kinds.

He did not ignore the affective side of life nearly as much as is commonly imagined; and if he held that the current anxiety to explain almost everything in terms of feeling is symptomatic of the excitement of a war and a post-war period, he was not alone in that view.

It is useless to speculate as to what might have happened if he had held his psychology more free from his metaphysics. As it is, the first to him was a preface to the second, and it can hardly be doubted that in some instances the text was allowed to dictate the introduction. But he, first among English psychologists, clearly saw the mental life as an integrative process throughout. In this he was not merely in line with the best of the more recent developments in the scientific study of life and mind, but he may be said to have prepared the way for those developments and to have contributed powerfully towards them.

I should like to bring this brief notice to a close by a few remarks about Ward as a teacher. The first time I ever saw him was at a lecture, and I remember it well. He came slowly into the small room—there were only about eight or nine students—his long, spare form struggling into his gown, his very keen and penetrating eyes taking us all in. It was the first lecture of the year, and there was a certain ritual to go through before he could begin to talk. He must find out who we were, and what we proposed to do, and he must discourage us a little so as to put us into a proper frame of mind. "Well, I don't know why you come to me; I don't know what you expect to get out of me; but whatever it is I expect you'll be disappointed." Then he sat down. Out came half a sheet of note-paper which he by no means needed, and for an hour he talked and we were literally held in spell. There was no hesitation, the right word seemed always to come, the illustrations were frequent, brilliant and human, the asides and reminiscences were full of fun. There was little formality. Nobody could do much in the way of taking notes. But I do not think he ever sacrificed a serious point to make a quip, although he never strangled humor to preserve solemnity.

That was not all. We met him at other times in his own home. Then, when he was in the vein, he would talk, as few can do, about anything and everything, but best of all about flowers and trees, and birds and beasts and people. Two things shone through all the time. The first was his keen interest in life and its great march forward; the second was his belief in the value of the individual and his intolerance of all bars to freedom.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

L. Further Experiments on Directed Recall as a Test of Cheerful and Depressed Temperaments

By M. F. Washburn, Lilian Harding, Henrietta Simons, and Dorothy Tomlinson

In this Journal, 30 (1919), 302-304, there were reported some experiments from our laboratory whose object was to see whether any correlation would appear between temperamental tendencies to cheerfulness or depression, and tendencies to recall pleasant or unpleasant ideas in connection with verbal stimuli, when the *O* was instructed to continue thinking, after the stimulus word was given, until either a pleasant or an unpleasant idea occurred to her, in case the first suggestion was neutral in affective tone. The results showed enough correlation with estimates of the *O*s' temperamental tendencies by their friends to encourage further study of the method.

In the present investigation, we proceeded in the following way. Each of the members of a large class in psychology was asked to judge herself by using one of the four terms: steadily cheerful; variable tending to cheerfulness; variable tending to depression; steadily depressed. From this large group, two approximately equal groups were formed, one containing those who had rated themselves as steadily cheerful, the other those who had judged themselves to be steadily depressed and those who had called themselves variable tending to depression. There were only very few who classed themselves as steadily depressed; hence the depressed group had to include the 'variable-depressed' class. The cheerful group numbered 33; the depressed group, 34.

In addition to these self-judgments, for each *O* the judgments of three friends were obtained, using the same descriptive terms. It was then possible to rate the *O*s numerically with regard to cheerfulness. The judgment 'steadily cheerful' was counted as four points; 'variable tending to cheerfulness' as three points; 'variable tending to depression', two points; 'steadily depressed', one point. If an *O* was judged steadily cheerful by herself and all three of her friends, her cheerfulness was rated at 16 points, that is, 4 4-point judgments; if considered steadily cheerful by herself and two of her friends and variable tending to cheerfulness by the third friend, her total rating was 15 points; and so on.

The experimenting was done as follows. Three series of 50 stimulus words each were prepared, an effort being made to avoid words with an obvious pleasant or unpleasant suggestion. An *O* was given one of these 3 series on each of 3 consecutive days. In the 1919 investigation there were 5 series, used on 5 consecutive days. The reason for experimenting on more than one day was of course to minimize the influence of temporary mood. The preliminary instructions were as in 1919: "When I pronounce a word to you, observe what idea that word first calls to your mind, and report whether it is a pleasant or unpleasant idea. If it seems neither pleasant nor unpleasant, but indifferent, continue thinking until a pleasant or an unpleasant idea is suggested and report which it is."

The results were scored for each *O* in terms of the total number of pleasant associations she experienced in the 3 series.

Results

(1) *In groups based on self-judgment.* We have seen that our original selection of *Os* comprised those who judged themselves as steadily cheerful, 33 in number; and those who judged themselves as either steadily depressed or variable tending to depression, 34 in number. In these two groups combined, the median score in number of pleasant associations was 97.5. Of the 33 in the cheerful group, 21, or 66%, had more than the median number of pleasant associations. Of the 34 in the depressed group, 22, or 64.7%, had less than the median number of pleasant associations.

The highest quartile in number of pleasant associations included those who had scores of 114 and over. Sixteen out of 17 or 94% of the *Os* who fell in this quartile belonged to the cheerful group. The lowest quartile included those who had scores of 80 and less. Twelve out of the 17 belonging in this quartile, or 70%, were in the depressed group.

(2) *In groups based on self-judgment plus judgment by others*; that is, on cheerfulness scores as above explained. The median cheerfulness score was 12.5 points. Of those who were above the median score in number of pleasant associations, 21, or 62.6% of half the number of *Os*, were above the median cheerfulness score. Of those who were below the median in number of pleasant associations, 22, or 65.6%, were below the median cheerfulness score.

The highest quartile in cheerfulness scores included those with scores of 15 and 16 points. Eleven out of the 17 *Os*, or 64.7%, who were in the first quartile in number of pleasant associations, were also in the first quartile in cheerfulness scores; 13, or 76.4%, were above the median. The last quartile in cheerfulness scores could not be found, as there were 15 persons with scores of 9 or less and 20 with scores of 10 or less. Of the 15 with scores of 9 or less, only 33% were in the last quartile, and 60% below the median, in number of pleasant associations.

It thus appears that, if we use the median score in pleasant associations as the dividing line, we shall find about two-thirds of the cheerful group lying above it. There is less correlation between the depressed group and the number of pleasant associations. This may well be due to the fact that our depressed group was largely composed of persons who had rated themselves not 'steadily depressed' but 'variable tending to depression', while the members of the cheerful group had all rated themselves 'steadily cheerful'. Or, to put the matter more accurately in terms of cheerfulness scores, the median for cheerfulness scores ranging from 4 to 16 inclusive in a group of 67 persons should have been about 10.9, and our actual median was 12.5.

Some Galvanometric Tests

In the case of about half our *Os* we took readings with a d'Arsonval galvanometer during the experiments. Our procedure was the simple one of tying cylindrical electrodes securely to the palms of our *Os*' hands, who sat in a comfortable position, and recording the maximum change of position of the bright disk that occurred while the *O* was recalling the pleasant or unpleasant experience. These deviations for pleasant and unpleasant recalls were recorded in separate columns, and for each *O* and each set of stimulus words the average 'pleasant' deflection and average 'unpleasant' deflection were found. For the cheerful group of *Os* 93 such averages were obtained, of which 40% were of more than 10°. For the depressed group, 72 averages were found, of which only 4.1% were more than 10° in extent. In the cheerful group, 35% of the average deflections were of 5° or less; in the depressed group 66% were of 5° or less.

Of the 10° or more average deflections in the cheerful group, 17 were averages for pleasant associations and 21 were averages for unpleasant associations. Of the 3 cases of averages amounting to 10° or more in the depressed group, all were averages for pleasant associations. Of the 5° or

less average deflections in the cheerful group, 16 were averages for pleasant associations and 17 were averages for unpleasant associations. Of the 5° or less average deflections in the depressed group, 25 were averages for pleasant associations and 23 were averages for unpleasant associations.

These figures, as far as they go, suggest the following conclusions with regard to the galvanometer effects: (1) the cheerful group of *O*s produces more effect on the galvanometer than does the depressed group; (2) there is no difference in effect between pleasant and unpleasant associations.

LI. Memory Revival of Emotions as a Test of Emotional and Phlegmatic Temperaments

By M. F. Washburn, Frederica Giang, Margaret Ives, and Martha Pollock

A. Revival of Single Incidents

Two previous Studies from this laboratory (this Journal, 34, 1923, 99ff.; 35, 1924, 113ff.) dealt with the process of recalling emotions, and compared the intensity of recall, the speed of recall, and the remoteness in time of the original experience, in the case of different emotions. In the present Study our aim is to see whether the method used in these earlier investigations may not furnish some data for the psychology of individual difference; in particular, whether it can serve to some extent as a test of emotional and phlegmatic temperaments.

At the outset, the members of a large class in psychology, all young women, were asked to judge themselves, using one of the following terms: emotional; rather emotional than calm; rather calm than emotional; calm. On each of those who complied with this request, the judgments of 3 friends were obtained, the same terms being used. Where the judgment 'emotional' was made on an individual, either by herself or by one of her friends, 4 points were credited to her score of emotionality (if the word may be pardoned); when the judgment 'rather emotional than calm' was made, it counted 3 points; 'rather calm than emotional' counted for 2 points, and 'calm' 1 point. Thus an individual's score might be anything between 16 (4 judgments of 'emotional'), and 4, produced by 4 judgments of 'calm'.

There were 3 *O*s whose total score of emotionality was 16 points each; one whose score was 15 points; 3 whose scores amounted to 14 points each; 11 whose scores were 13 points each; and 11 whose scores were 12 points each. There were 3 *O*s whose scores were 4 points each; 4 whose scores were 5 points each; 12 whose scores were 6 points each; 6 whose scores were 7 points each, and 9 whose scores were 8 points each. It will be seen that we could not, without a random selection from the 8-point group, make equal groups of emotional and calm *O*s. We experimented on all of the above *O*s; that is, on an emotional group of 29 comprising those with scores from 16 to 12 inclusive, and on a calm group of 34, comprising those with scores of from 4 to 8 inclusive. To obviate the disadvantage of inequality between the groups, we stated our results in terms of percentages of each group.

Each *O* was experimented on according to the method used in the Studies above referred to: that is, she was given the following directions: "I want you to recall some occasion when you were very angry, (or "felt great joy," or "felt very much frightened"). Let the emotion develop again as far as it will, so that as far as possible you relive the incident and its accompanying emotion. Do not speak, but give a signal when you feel the emotion as fully as you can in this revived form."

A stop-watch was started when the *O* began to fulfill these instructions, and stopped when she signalled that the recall was complete. She was then asked whether the revived emotion was as intense as the original one, in which case it was credited with 4 points of intensity; whether it was somewhat less intense, counting 3 points; very much less intense, counting 2 points; or whether there was merely the recollection that the emotion had once been felt, which counted 1 point. She was also asked how long ago the original incident had occurred. Each *O* thus recalled an incident causing anger, one causing joy, and one causing fear.

Results

I. Intensity of Recall. (*a*) *Anger.* There were 19 cases of the revival of anger at intensity 1, the weakest grade. Of these, 8 were made by the *Os* of the emotional group, constituting 27.5% of that group; and 11 by members of the calm group, constituting 32.3% of that group.

There were 27 cases of the revival of anger at intensity 2: of these 10 were made by emotional *Os*, or 34.4% of that group, and 17 by calm *Os*, or 50% of that group.

There were 18 cases of the revival of anger at intensity 3: of these 10 were made by emotional *Os*, or 34.4% of that group, and 8 by calm *Os*, or 23.5% of that group. There was no case of the recall of anger at intensity 4.

It thus appears that, while there is some tendency for the calm *Os* to recall anger with less intensity, the difference is far from being sufficient to separate the groups.

(*b*) *Joy.* There was only one case of the recall of joy at the lowest intensity, 1. This was by a member of the calm group. In 16 cases joy was recalled at intensity 2; 3 of these recalls were made by emotional *Os*, or 10.3% of the emotional group; 13 by the calm group, making with the *O* who recalled joy at intensity 1, 41.1% of the calm group recalling joy at an intensity less than 3. In 43 cases joy was recalled at intensity 3; these comprised 25, or 86.2%, of the emotional group, and 18, or 52.8%, of the calm group. In 3 cases, one emotional and two calm, an intensity of 4 was reported. Summarizing these results: 10% of the emotional group and 41% of the calm group revived joy with an intensity of 2 or less: 80% of the emotional group and 59% of the calm group revived it with an intensity of 3 or more. The best dividing line for test purposes, so far as these results go, would be revival at an intensity of 2 or less; a group thus formed would contain about 4 times as many calm as emotional *Os*.

(*c*) *Fear.* Fifteen persons revived fear with an intensity of only 1: 7 of these were emotional *Os*, 24.15% of the emotional group, and 8 were calm, 23.5% of the calm group. Twenty-five persons revived fear with an intensity of 2: 12 of these were emotional, 41.3% of the emotional group, and 13 were calm, 38.2% of that group. Twenty persons revived fear with an intensity of 3: 8, or 27.6%, of the emotional group and 12, or 35.3%, of the calm group. Three persons, two emotional and one calm, revived fear with an intensity of 4. To summarize: 24% of the emotional group and 23% of the calm group revived fear with an intensity below 2: 34% of the emotional group and 38% of the calm group revived it with an intensity above 2.

Reviving joy at an intensity of 2 or less is thus the only approximation to a diagnostic mark to be obtained from introspective reports of the intensity of a revived emotion of anger, joy, or fear.

II. Speed of Recall. As was noted in the earlier Studies made on revived emotions, two individuals can be only roughly compared in regard to the speed with which they recall emotions in these experiments, for we cannot be sure that they will consider that the same degree of development of

revived emotion constitutes that full development at which they are instructed to give the signal. Our results show no significant difference between the emotional and the calm groups as regards speed of recall.

III. Remoteness of Original Experience. (*a*) *Anger.* Twenty-seven *O*s recalled anger from a date less than one year before: 18 of these were emotional, constituting 62% of the emotional group; 9 were calm, constituting 26.4% of the calm group. Twenty-three *O*s recalled anger from a date two years or more before: 6 of these were in the emotional group, constituting 20.6% of that group; 17 were calm, constituting 50% of the calm group. Thirteen *O*s recalled anger from a period of more than 5 years before the time of recall: 2 of these were emotional, that is, 6.9% of the emotional group, 11 were calm, that is, 32.3% of the calm group.

It appears that there is a fairly well-marked difference between the groups as regards the recency of the anger-incidents recalled. The proportion of calm *O*s recalling anger from two or more years is more than twice as great as that of the emotional *O*s recalling from the same time-distance: for a remoteness of 5 or more years the proportion of calm *O*s is nearly 5 times that of the emotional *O*s.

(*b*) *Joy.* Here the diagnostic mark is recall from very recent date. There were 17 *O*s who recalled joy from a period less than one month before: of these 11 were emotional, constituting 37.9% of the emotional group, and 6 were calm, constituting 17.6% of the calm group. Recall from a remote period was about equally distributed between the two groups: 13 emotional *O*s recalled joy from a period of over 6 months before, constituting 47.8% of the emotional group, and 17 calm *O*s, or 50% of the calm group, did likewise.

(*c*) *Fear.* Here also recall from a recent period was fairly diagnostic. Twenty-six *O*s recalled fear from a period of less than one year; of these, 18 were emotional, constituting 62% of the emotional group, and 8 were calm, constituting 23.5% of the calm group. Twenty-eight *O*s recalled fear from a period of more than one year; of these 10 were emotional, constituting 34.4% of the emotional group, and 18 were calm, constituting 52.9% of the calm group.

B. The Number of Emotionally Toned Incidents Recalled. If an *O* is asked to recall as many occasions as he can when he was angry, or felt joy, or was frightened, will the number be greater if the *O* belongs to the emotional group?

Thirty persons whose emotionality score was 12 or above, and 35 whose emotionality score was 8 or less, were experimented on as follows. They were each asked to recall as many instances as they could when they had been very angry, and to give a signal as each one occurred to them. A stop-watch was started when they began the process of recall, and stopped when they declared they could think of no more cases: the number of signals given and the total time occupied in recall were recorded. The same procedure was followed for the recall of joy and fear.

Results

In the emotional group 11 persons, or 36% of the group, recalled more than 10 *angers*: in the calm group 9 persons, or 25% of the group, recalled more than 10 angers.

In the emotional group 5 persons, or 16% of the group, recalled fewer than 5 angers: in the calm group 17, or 50% of the group, recalled fewer than 5 angers.

In the emotional group 16 persons, or 53% of the group, recalled more than 20 *joys;* in the calm group 19 persons, 54% of the group, recalled more than 20 joys.

In the emotional group 4 persons, 13% of the group, recalled fewer than 10 joys; in the calm group 15 persons, 43%, recalled fewer than 10 joys.

In the emotional group 11 persons, 36%, recalled more than 15 *fears*; in the calm group 6 persons, 17%, recalled more than 15 fears.

In the emotional group 4 persons, 13%, recalled fewer than 5 fears; in the calm group 11 persons, 31%, recalled fewer than 5 fears.

Considering the total of emotional recalls: in the emotional group 16, 53%, recalled more than 40 cases of emotion: in the calm group 17, 48%, recalled more than 40 cases.

In the emotional group 3, 10%, recalled fewer than 20 cases of emotion: in the calm group 13, 37%, recalled fewer than 20 cases.

In the emotional group 5, 16%, recalled fewer than 30 cases of emotion: in the calm group 21, 60%, recalled fewer than 30 cases.

These results so far as they go indicate the following conclusions. (1) A larger proportion of emotional than of calm *Os* recall many angers and many fears: about the same proportion of emotional as of calm *Os* recall many joys. This fact, along with the absolutely greater number of joys recalled, should be considered in connection with the highly favored situation of our *Os*, all young, well, and in good economic circumstances.

(2) A decidedly larger proportion of calm than of emotional *Os* recall few angers, joys and fears.

(3) The best diagnostic test is a total recall of fewer than 30 emotions. The proportion of calm *Os* recalling this number is more than 4 times as large as the proportion of emotional *Os*.

LII. Galvanometer Experiments with Revived Emotions as a Test of Emotional and Phlegmatic Temperaments

By M. F. Washburn and Frederica Pisek

Two groups of 21 *Os* each, a calm group having emotionality scores of 7 or less, and an emotional group having scores of 12 or more, were experimented on as follows. The *O* was comfortably seated, and cylindrical brass electrodes attached to a d'Arsonval galvanometer were securely tied to the palms of her hands. She was then asked to recall anger, joy, and fear as reported in the preceding Study, and to report the intensity of the revived emotion and the remoteness of the original experience. The *E* noted the extreme deflection of the galvanometer during the process of recall.

When the maximum deviations for all the recalls obtained were arranged in order of their size, it was found that, among those *Os* who gave deflections above the median amount, 80% were emotional and 20% calm: for *Os* giving deflections below the median amount these figures were of course reversed.

Of the *Os* giving deflections in the highest quartile, 90% were emotional: of those giving deflections in the lowest quartile, 70% were calm.

There were 21 *Os* whose maximum deflections for the three emotions, anger, joy, and fear, averaged above 10°. Of these 80.9% belonged to the emotional group.

There were 12 *Os* in whose cases a similar average fell below 5°. Of these 75% belonged to the calm group.

Incidentally, it is interesting to see whether and how far the galvanometer disturbance confirms the *Os'* report as to the intensity of the revived

emotion. In the case of the calm group, no correlation appears. If we select in the case of each *O* that recall which gave the largest galvanometric deflection and record the degree of intensity (1, 2, 3, or 4) which she introspectively ascribed to it, the average of such intensities is for the calm group 2.02. If we select, similarly, the recalls which gave the smallest galvanometric deflections and average their intensities, we find that for the calm group this average is 2.18; a difference in the wrong direction, though negligible in amount. In the case of the emotional group a positive correlation appears. The average intensity of their recalls giving maximum deflections is 2.79; of those giving minimum deflections it is 2.17.

Possibly an emotional person is a better introspective judge of the intensity of emotions, since he experiences them oftener and through a higher range of intensities.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

IV. On Inhibition of the Differential Threshold in Accordance with Heymans' Law

By John E. Anderson

In 1899 Heymans[1] formulated a general law of inhibition, which stated that the inhibitory power of a stimulus, measured by the stimulus whose effect it can just completely inhibit, increases in direct proportion to its intensity. By a series of very careful and complete researches, during the following years, he was able to demonstrate the validity of this law in a number of sense-fields and to work out many of its implications, including its application to the differential threshold. The major part of his work was done with a single *S*.

Spencer[2] recently took up the problem and was able to demonstrate that the quantitative relation held for the simple threshold of the white rat. He measured the simple threshold of 5 rats for a white light, in the absence of and in the presence of another or inhibiting light (the inhibiting light being of two intensities, one double the other), and found that the simple threshold was a linear function of the intensity of the inhibiting stimulus.

In Heymans' terminology, the inhibiting or concurrent stimulus is called the active stimulus, and the inhibited stimulus (the one for which the thresholds are obtained) is called the passive stimulus. His law states that the threshold for the passive stimulus increases directly with the intensity of the simultaneously presented active stimulus.

Heymans, in his original work, used specially designed rotating discs for both active and passive stimuli, obtaining the thresholds by the method of minimal changes. Later some of his work was done with slide photometers. For our purposes the slide photometers seemed to offer much greater ease of adjustment. A device was constructed consisting of three parallel slide photometers, in blackened tunnels, which illuminate apertures covered with milk glass. When the slides are moved toward or away from the apertures the intensity of the light increases or decreases in accordance with the law of inverse squares.

When the *S* seated in a dark room looked into a hood 1 m. from the apertures, he saw before him 3 circular fields of light, a central one 3 cm. in diam. (the active stimulus) on either side of which were two smaller fields, each 1 cm. in diam. (the passive stimuli). The distance between the centers of the fields was 7.3 cm. His task was to observe the two small fields and to report the instant he noticed any change in intensity by stating which of the two was the brighter. At the beginning of the judgment, the two outer lights were objectively equal in intensity. The *E* moved the slide corresponding to one of the two outer lights away from the *S* a unit at a time, thus decreasing the intensity. The slide was marked off in logarithmic units, so that the intensity of the light could be decreased by uniform steps. When the judgment was made, the *E* stopped moving the slide,

[1] G. Heymans, *Z. f. Psych. u. Phys. d. Sinnesorg.*, 21, 1899, 321-359; *Z. f. Psych.*, 53, 1909, 401-415.

[2] L. T. Spencer, *Jour. Comp. Psych.*, 3, 1923, 389-408.

and turned a switch which cut off all three lights. After recording the position and resetting the slides, he turned on the switch, the click being the signal that the apparatus was ready for the next judgment. The method is, then, one of the continuous change of one stimulus moving from objective equality in the direction of a lower intensity, the additional requirement being set up that the judgment which stimulus was the brighter must be correct. As the *Ss* were cautioned to be relatively sure of their judgments practically no incorrect judgments were made, the maximum number of errors being 8 by one *S* in 100 judgments. When an incorrect judgment was made the trial was repeated at the close of the series. Fifty judgments were made a day, in 5 series of 10 each, the series being separated by a rest period. A preliminary practice period was given before the experiments, and a short practice was given before each experiment. Each series of 10 judgments contained 2 judgments at each intensity of the active stimulus, the order of appearance for the different intensities being arranged so that practice and adaptation effects due to the temporal arrangement were balanced. The standard passive stimulus remained at the same intensity throughout each experiment.

Measurements of the intensity of stimuli and of the thresholds are expressed in terms of comparable light-units, a 60 watt Mazda lamp at 10 cm. from the milk glass aperture being designated 1,000 units. The slides of the photometers were marked off in these units. The source-lights were three 60 watt tungsten lamps, between which the maximum difference was .050 light-units. Two sets of experiments were made, the first set on 3 *Ss*, with the center light or active stimulus at 40, 80, 120, 160, and 200 units respectively, with the standard outer light or passive stimulus at 111 units. Although 2 of these active stimuli are below the intensity of the passive stimulus as measured in light-units, the greater area of the active stimulus prevents us from concluding that the stimulating value in these cases is less than that of the passive standard stimulus. The second set of experiments was done on 5 other subjects, with the active stimulus at 100, 200, 300, 400, and 500 light-units respectively and the standard passive stimulus at 50 units. In the first set of experiments each *S* worked one day until a total of 50 judgments or 10 for each intensity of the active stimulus was obtained. In the second set of experiments each *S* worked 2 days, so that a total of 100 judgments or 20 for each intensity of the active stimulus was obtained.

The results of the experiments are presented in the following Tables.

Experiment 1. Differential Thresholds with Standard Passive Stimulus at 111 Light-Units

Intensity Active Stimulus	40	80	120	160	200
Mean (3 *Ss*)	19.7	22.9	25.8	27.6	31.3
Straight Line of Best-Fit Measures	19.86	22.66	25.46	28.26	31.06

Experiment 2. Differential Thresholds with Standard Passive Stimulus at 50 Light-Units

Intensity Active Stimulus	100	200	300	400	500
Mean (5 *Ss*)	9.11	9.96	10.78	11.95	12.99
Straight Line of Best-Fit Measures	9.00	9.98	10.96	11.94	12.92

For the line of best-fit values of the first experiment, the differential threshold with the standard passive stimulus at 111 light-units is increased by 2.8 light-units for each increase of 40 light-units in the active stimulus from 40 to 200 light-units. In the second experiment with the standard passive stimulus at 50 light-units, the differential threshold is increased by .98 light-units with every increase of the active stimulus by 100 light-units between 100 and 500 light-units.

As an incidental check on the reliability of the values obtained, the differential threshold with no active stimulus present was calculated by extending the line of best fit to the zero abscissa. For the first experiment with the standard stimulus at 111 light-units a differential threshold of 17.06 light-units is thus obtained; and for the second experiment with the standard stimulus at 50 light-units, a threshold of 8.02 light-units. In the first experiment the threshold is 15.34% of the standard stimulus, in the second 16.04% of the standard stimulus,—a striking agreement in the two experiments as to the ratio of the differential threshold to the stimuli compared.

The introspective reports of the *Ss* are interesting, in that several were unaware of the fact that the central light or active stimulus had changed at all during the series of experiments, but believed that the central light had remained constant while both the passive stimuli changed.

Summary and Conclusion, The differential threshold for light-stimuli in the presence of an inhibiting stimulus was measured by the method of continuous change. The results obtained on 8 *Ss* show that the differential threshold varies directly with the intensity of a concurrently presented visual stimulus within the range of stimuli studied.

BOOK REVIEWS

La psychologie des femmes. By G. HEYMANS. Tr. by R. LE SENNE, avec preface du traducteur. Paris, Félix Alcan, 1925. pp. xl, 315.

This is a translation of the second Greman edition (1924) of Heymans' *Die Psychologie der Frauen* which first appeared in 1910. The second edition (which we have not seen) is said to be "carefully and completely reviewed by the author, and brought into line with certain recent investigations." A comparison, however, of the first edition with this French translation shows little change; the only new title in the bibliography is O. Lipmann, *Psychische Geschlechtsunterschiede*, and the author's original and cautious conclusion remains the same. Although the book is based upon results obtained by the biographical method, its background is furnished by the earlier investigations of Heymans and Wiersma on heredity, and on the development of character in children of high-school age, in which the questionary method (*la méthode d'enquete*) was employed. For the convenience of the reader, both questionaries were published in an appendix to the first edition, and are now translated into the French.

Apart from the excellent translation, the particular feature of this French edition is La Senne's introduction of 40 pages, which is a study of Heymans' 'Special Psychology', particularly as regards its logical setting, its two principal procedures, and the specific nature of its subject-matter. The purpose of this summary is to furnish an account of, and to create an interest in, the work of *l'école de Groningue* taken as a whole. Le Senne's point of view is philosophical; he has caught the spirit of Heymans' work and, while enthusiastic, writes about it with intelligence and restraint. Although he has little to say of *La psychologie des femmes*, he has, nevertheless, furnished an admirable, if unusual, introduction to the work.

H. P. W.

Psychology: In Theory and Application. By H. W. DRESSER. New York, Thomas Y. Crowell Company, 1924. pp. 727.

The subtitle of this voluminous work is more significant than are subtitles usually. The book is an attempt to set forth from a scientific point or view the theory of human nature, and to show how this theory is being applied to certain aspects of life. The book is not suitable as a textbook, but it should form a foundation for the reading of every professor, student, or layman interested in psychology. Its value to a library is inestimable. In fact, the book is encyclopedic in nature, and since this is the case, it is natural that the author should bring together the views of most of our leading psychologists and philosophers. Rarely, if ever, does he take a definite stand on any controversial question; instead, he presents impartially the views of leading men and leaves the reader to draw his own conclusions. In the preface he emphasizes "the need of a book which approaches the task in the spirit of science for the sake of coordination, but without the partisanships which too readily prejudice either the theory or its practical values."

The divisions of the book are five in number. Part I is devoted to a brief treatment of general psychology, and is based largely on James, Warren, and Woodworth. Part II, called the "Psychology of the Hidden Self," offers a psychoanalytical treatment. The most frequently quoted authors are Brill, Ewer, Freud, Jung, Prince and Rivers. The subject-matter of Part III is vocational and industrial, with frequent reference to Hollingworth and Scott. This section contains two excellent chapters,

one on vocational guidance and one on advertising and selling. One chapter is devoted to mental tests; but the author is content to tell of the army results, while the practical question how tests are, or may be, applied to business and industry is hardly more than mentioned. "Social Psychology" makes up the fourth Part, and the fifth Part is a sociological treatise headed "Social Organization." The names of Ellwood, Hobhouse, McDougall, Royce, Santayana, Trotter, and others are sprinkled throughout the text and appear in the selected reference-list which follows each chapter.

Pennsylvania College for Women J. S. KINDER

Reklame-Psychologie (2nd Edition). By Theodor König. München and Berlin, R. Oldenbourg. 1924. pp. vii, 224.

The Psychological Institute of the University of Würzburg under Marbe's direction has been carrying on practical work in advertising; and in this book Theodor König, an associate of the Institute, attempts a systematic treatise on the subject. The greater part of the book is devoted to a description and discussion of the experimental work in psychology which has a direct bearing upon advertising and of experiments carried on in advertising. In this field, comparison with books published in this country is inevitable; and, as in our texts, the topics of perception, attention and association receive the greatest attention. In the main the results are in accord with the studies carried on in America and reported by Starch, Hollingworth and Kitson. There is, however, in this text as little reference to research conducted in this country as there are references in the American texts to the work carried on in Germany. A new edition of König is promised that will bring it up to date as regards American research. On the whole, the work is satisfactory, especially in the importance which it attaches to methodology, a topic which tends to be ignored in the American literature of the subject.

The writer is somewhat surprised to find that König disregards the experimental work in visual perception and movement carried on by Jaensch and by the members of the "Gestalt" school. In particular, chapters upon the size and form of objects and the spatial conditions of the stimuli would offer a very fertile field for the application of these newer experimental results. In so far as the writer knows, such application has not yet been made in the field of advertising.

Cornell University SETH WAKEMAN

Medical Education: A Comparative Study. By ABRAHAM FLEXNER. New York, The Macmillan Co., 1925. pp. ix, 334.

In this study of medical education from the comparative point of view Dr. Flexner has concerned himself largely, although not exclusively, with internal medicine. The study is, in itself, valuable as a well directed enquiry into the aims, methods and values of medical training. It is of more interest to the reviewer as a justification of the value of the comparative method of study in educational problems.

Cornell University SETH WAKEMAN

NOTES

The Control of Experimental Procedure

In reviewing the experimental work of Ernst *et al.*[1] (performed under my direction), Professor Robinson[2] takes exception to that part of the procedure in which certain stimulating conditions were eliminated because they introduced an additional variable. He says: "In general there is increasing recall with decreasing rate of presentation. But this is not strange, since as soon as it was discovered that the second subject did less well at the 1.25 sec. and 1.50 sec. rates than at that of 1.0 sec., these slower rates were discontinued for that subject. The arguments for discarding the slowest rates are that they give results in disagreement with what has been found by others and that, at those slow rates, a new factor of wandering attention was introduced. Are we to believe that attention wandering enters the situation suddenly after a given rate of presentation has been passed? Inversions of the second order appear in the functions for both observers."[3]

It would seem that Professor Robinson has failed to grasp the purpose of this Study,—although he does recognize that it was primarily methodological, for he remarks: "The real interest of these investigators apparently was not so much in the memory process as in the possibility of treating the data of a memory experiment by means of the refined statistical procedures of psychophysics."[4] In the experimental study of any group of phenomena which can be quantitatively treated, two main phases of investigation may be found. In the first stage, the variables are defined and their relationships are worked out in general terms. This is the stage of extensive experimentation and does not require great refinement of method. It is followed by a second stage of intensive investigation in which refinement of method is of utmost importance, and which is predicated upon the results of the extensive studies. In probably no field of psychology has the first stage been completed. Yet there are few fields in which some general relationships have not been sufficiently well worked out to warrant their intensive investigation. So it is with memory. There are many relationships between the numerous variable conditions affecting the memory-process concerning which we do not have even a general knowledge. If a failure on our part to investigate one of these unknown relationships is to be interpreted as a lack of interest in the memory-process, we must plead guilty. On the other hand, the various methods that have been used in the investigation of memory show (as must all pioneer and extensive work) many crudities. The time seemed ripe to take certain variables whose general relations were known and to apply to them, within a limited range, refined methods of intensive experimentation. This is what Ernst and his co-workers did. Two known relationships, those of number of repetitions and recall and of speed of presentation and recall, were chosen and studied intensively by means of an adaptation of psychophysical methods.

From this point of view the dropping of certain speeds of presentation became, under our conditions, an absolute necessity. A careful investigation of the relation between speed of presentation and amount of immediate recall required that all other factors be kept constant. We founded this investigation upon the general result which had been previously worked out, that the amount of recall decreases as the speed of presentation increases. We did not attempt to investigate whether or not this general

[1] J. L. Ernst, F. E. Smith, L. R. Moessner, E. S. Rudisill, and M. J. Atwater, Further Data for an Associative Limen, this Journal, 35, 1924, 255ff.

[2] E. S. Robinson, Memory, *Psychol. Bull.*, 21, 1924, 572f.

[3] *Op. cit.*, 573. [4] *Op. cit.*, 572.

relationship existed, but to measure it by certain methods. In so doing we built upon previous experimental work in this field. Only as more specialized experiments are predicated upon the results of more general experiments can a science advance. We then found that at certain speeds the general relationship which we were measuring did not appear to hold for one *O*. A new variable was suspected, and we were fortunate enough to obtain introspective evidence that it was a factor of attention. Had this clue not appeared, there would have been no justification for discarding any of the stimuli. But as soon as it was evident that an additional variable was influencing the results, it was also evident that the figures obtained at slow speeds would be incomparable with those obtained at high speeds unless we could in addition measure this new variable. As we were unable in the time at our disposal to investigate in detail the effect of attentional wandering, we could only keep conditions constant by avoiding those situations that appeared to involve it.

This leads to Professor Robinson's question whether we are to believe that the factor of wandering attention enters the situation suddenly at a given rate of presentation. That, of course, we do not know. The relation of speed of presentation to wandering attention is a separate problem requiring experimental investigation. If one should hazard a guess, one would expect that as the speed of presentation decreased the shifts in attention would affect a greater and greater percentage of associative connections. Where the effect may have started we did not know, since we did not attempt to measure it. As a matter of practical procedure, all we could do was to eliminate those situations in which we had any evidence that the effect was present. To have failed to eliminate them would not have added to our results but would have made the data worthless.

The writer is unable to understand whether or not Professor Robinson means that the presence of inversions of the second order in the curves obtained indicates that the attentional wandering may not have been completely eliminated. If he does, it should be pointed out that mathematically all curves that are ogival in type must contain a single point of inflection, that is, an inversion of the second order. This includes the curves we obtained for recall as conditioned upon number of presentations (where the attentional factor was not found) as well as the curves obtained in psychophysical studies of sensory qualities. The presence, then, of such inflections cannot be given any significance save on the basis of a more detailed analysis of 'mnemometric' curves, an analysis which must wait until further experimental results are available.

GILBERT J. RICH

INTELLIGENCE IN AN INTELLIGENCE TEST WITH AN AUDITORY DISTRACTOR

Although it is usual to asume that a distractor will adversely affect performance in an intelligence test, the assumption is scarcely safe without experimental demonstration. Supposedly adverse conditions do not always have as great an effect as might be expected (*e.g.*, loss of sleep[1]), and intended distractors are notoriously perverse in their frequent disposition to spur the attention and thus to improve performance.[2]

In 1923 the writer of this Note sought to test experimentally the effect of an auditory distractor upon the success of 56 students at Clark University in the Otis intelligence test. Two electric bells at different sides of the examination room were rung intermittently as the distractor.

[1]E. S. Robinson and S. O. Herrmann, *J. Exper. Psychol.*, 1922, 5, 19-32; E. S. Robinson and F. Richardson-Robinson, *ibid.*, 93-100.

[2]*Cf.* M. A. Tinker, this JOURNAL, 1922, 33, 578-583, and references to Münsterberg and Kazaki, Hamlin, Evans, and Cassel and Dallenbach there cited, 580.

The students were divided into two groups of 28 each. At the first session Group I was given Form A without distraction and Group II Form A with distraction. One week later Group I was given Form B with distraction and Group II Form B without distraction. Thus the tests with the distractor could be compared with those without the distractor with the elimination of constant differences between the two groups and of practice effects from the first to the second session.

There was an average gain in score with the distractor of 0.09%,—not a significant difference. On the average such a distractor neither aided nor hindered.

The general average, however, is here the least significant value. Apparently the better men were actually hindered by the distractor and the poorest men were aided. If the groups are divided into quartiles, the effect of the distractor is as follows:

Upper quartile	4.4% loss
Third quartile	0.5% gain
Second quartile	0.2% gain
Lower quartile	6.0% gain

Men of all quartiles lost in accuracy when the distractor sounded, but men of the upper quartile lost most (2.7%, as against 0.8% for the lower quartile). Hence in accuracy as well as in score the group gave a more uniform performance with the distractor.

From these general tendencies there are individual deviations. One man in the lower quartile gained 30% in score and another in the upper quartile lost 15% with the distractor. On the other hand, two men made the same score with and without the distractor. The coefficient of correlation of the scores with the distractor with those without it is $r = .86$ (P.E. = .02), and the corresponding correlation for accuracy is $r = .60$ (P.E. = .05).

The group is small, and the present finding can only be suggestive. It would be well to see if, with larger groups and with other kinds and degrees of distractors, a similar reduction of the dispersion of a group is found.

MILES A. TINKER

SANFORD'S *LABORATORY COURSE*

The publishers of Sanford's *Laboratory Course* tell me that they are willing to reprint a small edition of the book (to sell at $2.40), if I guarantee them a sale of one hundred copies. I hope that this offer may meet with a wide response; I shall be glad to receive orders for the reprint.

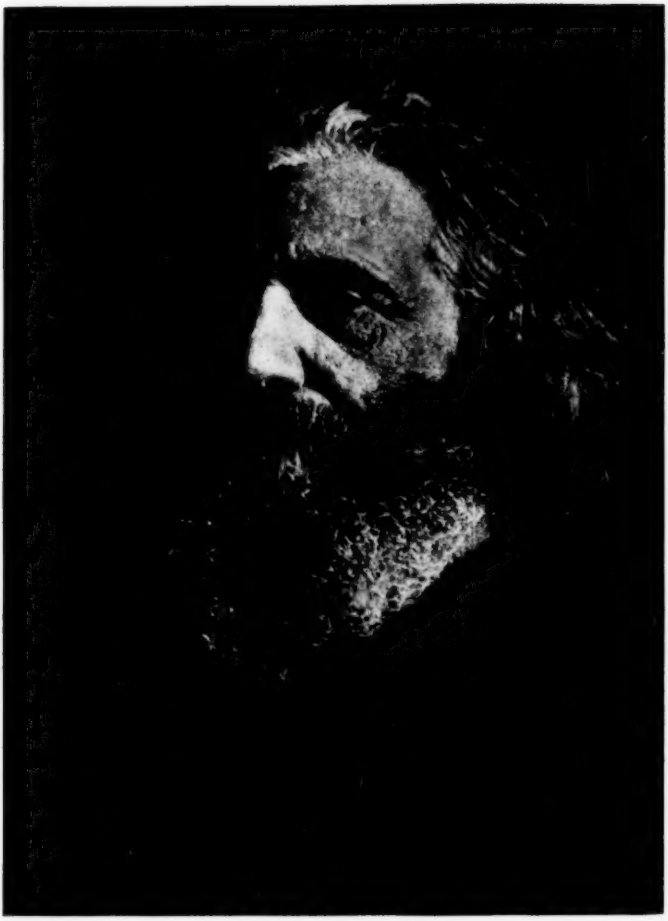
E. B. T.

EIGHTH INTERNATIONAL CONGRESS OF PSYCHOLOGY: FIRST ANNOUNCEMENT

The eighth international congress of psychology will be held at Groningen, Holland, Sept. 6-11, 1926. The congress will be limited to about two hundred well-known psychologists to be invited by the committee; psychologists who have not received an invitation, and who wish to attend the congress, are requested to apply to the First Secretary, Prof. Dr. F. Roels, Maliebaan 86, Utrecht, Holland. The membership subscription is 15 gulden; the recognised languages of the congress will be English, French and German.

Members of the committee of organization are Professor G. Heymans, president; Professor E. D. Wiersma, vice-president; Professors F. Roels and H. J. F. W. Brugmans, secretaries; and Professors L. Bouman, G. van Wayenburg and H. Zwaardemaker.

The photograph of Professor James Ward, of Cambridge University, which appears in this number, was taken by Messrs. Elliot & Fry, London.



J. P. Bentas